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VOL. CXI. JANUARY, 1881. No. 1.

ON THE EXPERIMENTS WITH THE PERKINS MACHINERY OF THE STEAM YACHT "ANTHRACITE."

By Chief-Engineer ISHERWOOD, United States Navy.

The *Anthracite* is the British steam yacht, 86 feet 4 inches long between perpendiculars, 16 feet 1 inch greatest breadth of beam, 10 feet 2 inches depth of hold, and 9 feet mean draught of water, that made a brief visit to the United States during the last summer for the purpose of exhibiting her machinery, which is constructed according to the system of Mr. Loftus Perkins, and embraces some novelties of mechanical detail, as well as the use of steam of higher pressure with greater expansion than has ever before been employed. The engine drives a 2-bladed screw of 6 feet 1 inch diameter and 9 feet pitch, and is peculiar in the compounding of its cylinders. The arrangement of the tubes of its surface condenser is also peculiar, and the design of the boiler is likewise new.

The superior economy of fuel claimed for this system is inferred from the exceedingly high steam pressure employed and the very great measure of expansion with which it is used, both being largely in excess of any previous practice. And, with the view of giving the fullest effect to the expansion, provision is made in the boiler for highly superheating the steam. In other words, the system of Mr. Perkins

is an attempt to realize practically the gain in economy inferable theoretically from the use of steam of higher pressure than heretofore employed and with a measure of expansion correspondingly greater. On these two hang all the law and the prophets of this system. There is nothing new in either, the system of Mr. Perkins being merely an extension of what has been in progress for many years. It embodies no new principles, but only endeavors to give those previously employed a more extended application by means of mechanical details better adapted for that purpose.

The steam is generated from water distilled from fresh water; and the wastage, inevitable in any case, is supplemented with water distilled from fresh water carried for that contingency, water distilled from sea water not being considered sufficiently pure for use with this system of machinery. The distillation is effected in a separate vessel. No lubricants are used in the valve chests and cylinders, but a bronze of peculiar composition and manipulation in manufacture is relied on without lubrication for the material of the rubbing surfaces of the steam valves and pistons, and, as the cylinders are vertical, the water lubrication, which in the engine of the *Anthracite* was excessive, in conjunction with this bronze appears to be sufficient to prevent abrasion. Experience with horizontal cylinders, or with those having but a slight water lubrication, might not be so satisfactory. The manufacture of this bronze is a trade secret.

Whether any gain in economy over the best existing practice can be obtained by employing steam of higher pressure with greater expansion is a question to be answered by experiment only, like any other question in physics. It cannot be answered inferentially from an abstract consideration of the properties of steam of high and low pressures, or from the gain due to the laws of expanding gases. The question must be directed to Nature, who alone is competent to give a correct reply, and it must be put in the language of experiment under the precise conditions in which the steam is to be used, so that the modifications effected by the material of the mechanism and by its mode of action may be included in the final result.

Only two experiments have been made with the machinery of the *Anthracite* to the knowledge of the writer; one in England on the 22d of May, 1880, by Mr. F. J. Bramwell, an expert employed for that purpose by the Directors of the Perkins Engine Company, whose report to them was reprinted in the September number of this journal

for 1880; the other was made by order of the Navy Department on the 13th and 14th of August, 1880, by a Board of Chief Engineers of the United States Navy, presided over by Chief Engineer Loring.

The results of the two are widely discordant. From the very able and complete report of Chief Engineer Loring to the Bureau of Steam Engineering of the Navy Department I have taken freely all the facts and calculations reproduced in this paper; and in a succeeding one I shall give the corresponding data and results of Mr. Bramwell's experiment, similarly arranged, the facts of which cannot be found in his report as printed in this journal, but which the writer has obtained from authentic drawings of the machinery and from the indicator diagrams given in a supplement to that report to the Directors and reproduced in their lithographic copy for distribution.

Before giving the data and results of the experiments it is necessary to preface them with a description of the machinery with which and the manner in which they were made.

Engine.

There is one engine of the vertical, direct-acting, inverted type, with three compounded cylinders of different diameters but same stroke of piston, the first two being single acting and the third double acting. The first or smallest cylinder is placed immediately above the second or next largest with their axes is the same vertical line. The third or largest cylinder has its axis parallel to the axes of the first and second cylinders, and is situated forward of them relatively to the vessel. The axes of all the cylinders are in the vertical plane passing lengthwise through the centre of the vessel. The casting of the first cylinder forms also the top cover of the second cylinder, the bottom of the first being open to or in common with the top of the second. The pistons of these two cylinders are connected by a casting of nearly the diameter of the first cylinder, and the space between the pistons is in common with the receiver. The first cylinder receives into its top the steam direct from the boiler, and its piston makes a downward stroke, at the end of which the steam is exhausted through a connecting pipe of 6·4918 square inches cross area and 29 inches length direct into the bottom of the second cylinder, whose piston makes the return upward stroke. From the second cylinder the steam is exhausted into a receiver from which the third cylinder is supplied alternately at top and bottom; this last cylinder exhausts into a surface condenser. The

engine thus, in effect, acts like an ordinary compound engine of two cylinders with an intermediate receiver.

The peculiar arrangement or combination of the first and second cylinders is for the same purpose as the compounding of the two cylinders of an ordinary compound engine, namely, to lessen the initial pressure on the crank pin, and to reduce the cylinder condensation by making the sum of the products of the difference of the temperatures due to the extreme pressures in the cylinders by the inner surfaces of the cylinders, less than in the case of a single equivalent cylinder giving the same mean pressure with the same boiler and final back pressure and the same measure of expansion. It is merely a further extension of the principle of the compound engine, being the same thing, only more so.

The first cylinder has at its top two double beat valves of the Cornish type, one for admitting the steam and cutting it off when a portion of the stroke of the cylinder is completed; the other for exhausting it at the end of the stroke into the second cylinder. This exhaust valve of the first cylinder is thus the steam valve of the second cylinder. The second cylinder has only one similar double beat valve and it exhausts the steam from the second cylinder into the receiver. The third cylinder has an ordinary three-ported slide valve with an expansion slide upon its back, the latter being worked from a prolongation upwards of the circulating pump rod. The valves of the first and second cylinders are worked by two eccentrics and a Stephenson link placed at one end of the crank shaft. The valve of the third cylinder is similarly worked by two eccentrics and a Stephenson link placed at the other end of the crank shaft. The reversal of the movement of the engine is effected by these links.

The Cornish valves are directly moved by lifting rods which rise and fall vertically, and whose upper end fit loosely into close-topped sockets made in the valves themselves, so that after the latter are seated the rods can recede from them. Cams working in yokes made in the lifting rods control the adjustment of these valves, whose upper faces are divided into two sections.

The cylinders and their covers are steam-jacketed in a peculiar manner. Coils of wrought iron pipe being properly placed in the mould, the metal of the cylinders and covers was cast around them and they thus became imbedded in its centre. The water of condensation in

the jackets was delivered into the condenser. The exterior of the cylinders and their covers was efficiently lagged.

From the bottom of the piston of the second cylinder there proceeds a piston rod whose lower end is secured into a crosshead moving between guides. From the cross-head journal the connecting rod continues directly to the crank pin. From the bottom of the piston of the third cylinder there proceeds a piston rod whose lower end is secured into a cross-head moving between guides, and from the journal of this cross-head a connecting rod proceeds directly to the crank pin. The crank shaft has two crank pin journals and four main journals.

The areas of the pistons of the three cylinders, exclusive of their rods, have the following proportions: Calling the area of the piston of the first cylinder 1·00000, the area of the piston of the second cylinder is 4·26212, and that of the third cylinder is 17·20179 including both sides of the piston as the third cylinder is double acting while the first and second cylinders are single acting.

In the first cylinder the steam is used for about the first half of the stroke of its piston without expansion, and during the remaining half expansively. In the second and third cylinders the steam is wholly used expansively, the cut off valve of the third cylinder acting only as a throttle valve to increase the back pressure against the piston of the second cylinder.

The circulating pump supplying the refrigerating water to the condenser is worked by a lever articulated to the piston rod of the third cylinder, and the air pump is worked by a similar lever similarly articulated to the piston rod of the second cylinder. The feed pumps and bilge pumps are worked by the cross-heads of the circulating pump and of the air pump. All the pumps are vertical and single acting.

The surface condenser is composed of galvanized wrought iron tubes, one-half of which have an inner diameter less than the outer diameter of the remaining half, so that those of the smaller diameter can be placed within those of the larger diameter and leave an annular space between when the axes of both coincided. All the tubes stand vertically and are arranged in pairs, a smaller and a larger diameter tube constituting a pair, with the smaller placed concentrically inside of the larger. Thus one-half of the tubes having the same diameter are outer tubes and the remaining half having the same diameter are inner tubes. The outer tubes have their upper end closed and their

lower end open and screwed into their horizontal tube plate. The inner tubes have both ends open and are screwed by their lower end into their horizontal tube plate, which is directly beneath the first tube plate. The upper open end of the inner tubes is within a very short distance of the upper closed end of the outer tubes. The refrigerating or injection water is forced by the circulating pump up the inner tubes and thence into the annular space between the inner and outer tubes down which it descends and passes overboard.

All the tubes are surrounded by the condenser shell, which forms, with the two tube plates, three compartments—one above the upper tube plate, one between the tube plates, and one beneath the lower tube plate. The upper compartment receives the exhaust steam from the third cylinder, which steam surrounding the outer tubes is condensed on their exterior surfaces, the water of condensation trickling down to the upper tube plate; this compartment communicates with the air pump which drains it. The lower compartment receives the refrigerating water from the circulating pump and distributes it to the inner tubes. The intermediate compartment, or the one between the tube plates, receives the refrigerating water from the annular spaces between the tubes of each pair, and passes it to the outboard delivery valve.

This arrangement of the tubes is exceedingly compact, makes perfectly tight joints with the tube plates, and allows free and independent expansion to each tube, but it has serious disadvantages as compared with the ordinary arrangement. It is greatly less accessible for examining, removing and replacing the tubes, few or many; in fact, these operations require the condenser to be taken apart; for, although the top of the shell is removable after the manner of a cover, yet, practically, the long flexible tubes, once firmly screwed into their plates and rusted there, could not be unscrewed by their upper ends, as intended by the designer. The power required to work the circulating pump is very much greater on account of the sharp reversal of the refrigerating water at the upper ends of the tubes, and on account of the resistance of the greater quantity of tube surface over which the water must be passed. The quantity of tube surface is nearly double, the surface of all the inner tubes being in excess of the surface required with the ordinary arrangement. This, also, in the same proportion, increases the weight of the condenser and the cost of its manufacture. The sole use of the inner tubes is to convey the refrigerating water to

the upper end of the outer tubes, only the exterior surface of which constitute the condensing surface; hence, all the inner tubes are in addition to what would be required with the ordinary arrangement to give equal condensing surface. The designer was doubtless led to the adoption of his arrangement of tubes by the perfection of the joint it afforded; for, with his system, any leakage of refrigerating water (which would be sea water) into the compartment receiving the water of condensation was inadmissible, as that much sea water would have passed into his boiler, but he paid dearly for this advantage.

The following are the principal dimensions and proportions of the engine:

Number of cylinders, . . .	3.
Diameter of the 1st cylinder (single acting),	$7\frac{3}{4}$ inches.
Stroke of the piston of the 1st cylinder, .	15 inches.
Area of the piston of the 1st cylinder, .	47·173 sq. inches.
Space displacement of the piston of the 1st cylinder, per stroke, . . .	0·4094884 cu. ft.
Space in clearance and steam passage of 1st cylinder, ...	0·0856481 cu. ft.
Diameter of the 2d cylinder (single acting),	$15\frac{13}{16}$ inches.
Diameter of the piston rod of the 2d cylinder, .	$2\frac{3}{4}$ inches.
Stroke of the piston of the 2d cylinder, .	15 inches.
Net area of the piston of the 2d cylinder, .	190·389 sq. inches.
Space displacement of the piston of the 2d cylinder, per stroke, . . .	1·6526823 cu. ft.
Space in clearance and steam passage of 2d cylinder, ...	0·3116325 cu. ft.
Diameter of the 3d cylinder (double acting),	$22\frac{13}{16}$ inches.
Diameter of the piston rod of the 3d cylinder, .	$2\frac{3}{4}$ inches.
Area of upper side of piston of 3d cylinder	408·700 sq. inches.
Net area of lower side of piston of 3d cylinder, .	402·760 sq. inches.
Mean area of the two sides of the piston of 3d cylinder, ...	405·730 sq. inches.
Stroke of the piston of the 3d cylinder, .	15 inches.
Mean space displacement of the piston of the 3d cylinder, per stroke, . , .	3·5219618 cu. ft.
Space in clearance and steam passage of 3d cylinder at one end, . . .	0·3194444 cu. ft.

Area of steam port of 1st cylinder ($1\frac{1}{4}$ by $2\frac{1}{2}$ inches =)	$3\frac{1}{8}$ square inches.
Area of exhaust port of 1st cylinder ($1\frac{1}{4}$ by 4 inches =)	5 square inches.
Area of exhaust port of 2d cylinder ($1\frac{1}{2}$ by $13\frac{1}{2}$ inches =)	$20\frac{1}{4}$ square inches.
Area of steam port of 3d cylinder (2 by $16\frac{3}{4}$ inches =)	$33\frac{1}{2}$ square inches.
Area of exhaust port of 3d cylinder (4 by $16\frac{3}{4}$ inches =) - . . .	67 square inches.
Depth of piston of 1st cylinder occupied by the packing rings, . . .	$3\frac{1}{2}$ inches.
Depth of piston of 2d cylinder occupied by the packing rings, . . .	$3\frac{1}{2}$ inches.
Depth of piston of 3d cylinder occupied by the packing rings, . . .	$4\frac{1}{8}$ inches.
Total depth of piston of 2d cylinder, .	5 inches.
Total depth of piston of 3d cylinder, .	$5\frac{5}{8}$ inches.
Thickness of metal of all the three cylinders,	$1\frac{1}{2}$ inches.
Total interior surface of the 1st cylinder exposed to the contact of the steam, including the upper surface of the piston and the surfaces of the steam passages and clearance, . .	5·09236 sq. feet.
Total interior surface of the 2d cylinder exposed to the contact of the steam, including the lower surface of the piston, half the surface of the piston rod, and the surfaces of the steam passage and clearance, . . .	13·06982 sq. feet.
Total interior surface of the 3d cylinder exposed to the contact of the steam, including both surfaces of the piston, half the surface of the piston rod, and the surfaces of all the steam passages and clearances, . . .	22·52750 sq. feet.
Outside diameter of the wrought iron pipe composing the coils of the steam jackets for the three cylinders, . . .	$\frac{5}{8}$ inch.
Inside diameter of the wrought iron pipe composing the coils of the steam jackets for the three cylinders, . . .	$\frac{3}{8}$ inch.

Distance between the axes of the coils of the wrought iron pipe composing the steam jackets for the three cylinders, ...	$1\frac{5}{16}$ inches.
Per centum of the interior surface of the cylinders steam jacketed, being the ratio of the inside surface of the wrought iron pipe composing the coils of the jackets to the interior surface of the cylinders. These coils are in both top and bottom of the cylinders as well as around the sides, . . .	60·80
Clearance of all the cylinders, . .	$\frac{3}{8}$ inch.
Inner diameter of the steam pipe from boiler, .	$1\frac{1}{8}$ inches.
Cross area of the steam pipe from boiler, .	0·994 sq. inch.
Ratio of cross area of steam pipe to area of piston of 1st cylinder, . . .	0·02107
Cross area of pipe connecting top of 1st cylinder with bottom of 2d cylinder, . .	6·4918 sq. inches.
Length of pipe connecting top of 1st cylinder with bottom of 2d cylinder . .	29 inches.
Ratio of the space displacement of the piston of the 1st cylinder in equal time to that of the piston of the 2d cylinder, . .	4·03597
Ratio of the space displacement of the piston of the 1st cylinder in equal time to that of the piston of the 3d cylinder, . .	17·20179
Ratio of the space displacement of the piston of the 2d cylinder in equal time to that of the piston of the 3d cylinder, . .	4·26212
Ratio of the aggregate displacements of the pistons of the 1st and 2d cylinders in equal time to that of the piston of the 3d cylinder, .	3·41578
Ratio of the space in clearance and steam passage of 1st cylinder to the space displacement per stroke of its piston, . . .	0·209159
Ratio of the space in clearance and steam passage of 2d cylinder to the space displacement per stroke of its piston, . . .	0·188561
Ratio of the space in clearance and steam passage of 3d cylinder to the space displacement per stroke of its piston, . . .	0·090707

Diameter of crank shaft, . .	$5\frac{1}{2}$ inches.
Condensing surface in condenser, . .	422 square feet.
Diameter of air pump (one, vertical and single acting), ...	$11\frac{1}{2}$ inches.
Stroke of air pump piston, . .	$4\frac{1}{2}$ inches.
Diameter of circulating pump (one, vertical and single acting), . . .	$11\frac{1}{2}$ inches.
Stroke of piston of circulating pump, .	$4\frac{1}{2}$ inches.
Diameter of feed pumps (two, single acting), .	2 inches.
Stroke of feed pumps' pistons, . .	$4\frac{1}{2}$ inches.
Diameter of bilge pumps (two, single acting), .	3 inches.
Stroke of bilge pumps' pistons, .	$4\frac{1}{2}$ inches.
Length in vessel occupied by engine and boiler, .	$22\frac{1}{2}$ feet.
Total weight of machinery, including engine, boiler, screw shaft, screw and all appurtenances except water in boiler, . . .	25 tons.
The tank in the engine room, carrying distilled water for supplying the boiler wastage, contains	280 pounds.

It will be observed from the above dimensions that the spaces in the clearances and steam passages of the cylinders are enormous, as compared with what are usually found in good practice, being from twice to four times greater than necessary, and involving a proportionally decreased economic effect from the steam. Although the Cornish equilibrium valves may be indispensable for working with steam of the high pressure intended in the first and second cylinders, yet they may be arranged so that the spaces in the clearances and steam passages shall not exceed 5 to 6 per centum of the space displacement of the respective pistons per stroke. And, certainly, the ordinary slide valve can be adapted to the third cylinder without requiring over 9 per centum of the space displacement of its piston per stroke in waste spaces.

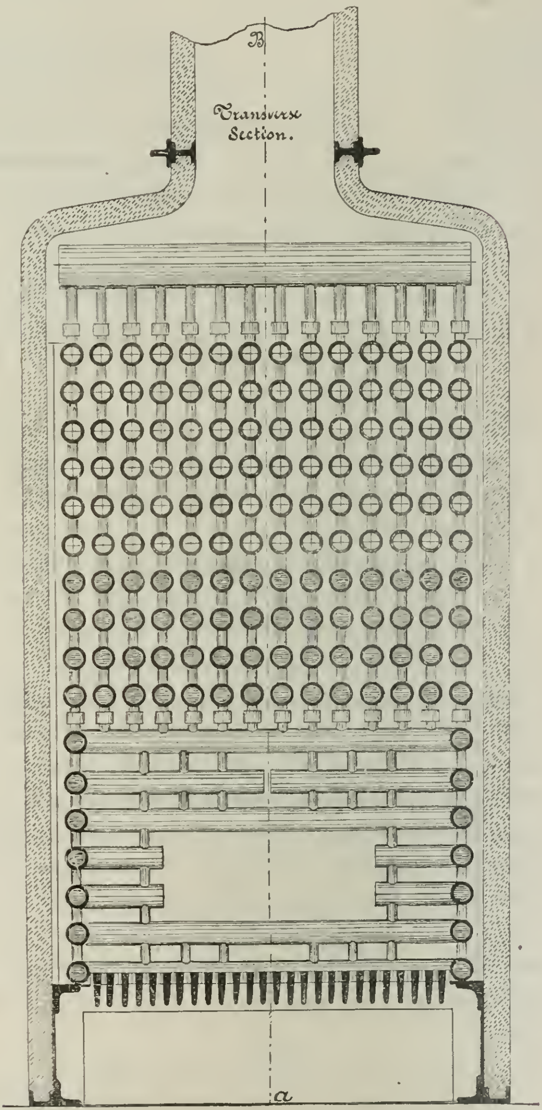
The disadvantage of the unusually large waste spaces at the ends of the cylinders is not limited to the loss of steam required to fill them, but extends to the loss of steam due to its condensation on the surfaces of these spaces. These losses of economic effect, greater in the engine of the *Anthracite* than is found in ordinary engines, are not inherent to the Perkins system, and may easily be eliminated by a better design of mechanical details.

The method of steam jacketing the cylinders is very inefficient, so much so, indeed, that probably but little economic effect is obtained from their use. The jackets, as already described, are composed of coils of wrought iron pipe embedded in the metal of the cylinder which was cast around them. By this arrangement the whole heating surface of the jacket is composed of the inside surface of the pipe. The steam from the boiler, being admitted at the top of the coil, follows it to the bottom, becoming in part condensed during the progress, the water of condensation filling the lower portion of the coil, which is consequently cooler than the boiler steam in the upper portion, and, to that extent, less efficient. The heat of the steam or of the hot water has to be accepted by the wrought iron pipe, which then delivers it to the cast iron of the cylinder, the heat wave being much impeded by this additional barrier. With the ordinary steam jacket, the boiler steam is at once in direct contact with every particle of the cast iron of the cylinder; the jacket has the maximum temperature, and the heat is communicated to the entire interior surface of the cylinder without the interposition of a single impediment. In the Perkins system, the heat-delivering surface of the steam jacket or coil is only 60·8 per centum of the interior surface of the cylinder, owing to the distance at which the coils are placed apart. With the ordinary steam jacket, the heat-delivering surface is co-extensive with the interior surface of the cylinder. In fact, the Perkins system of steam jacketing cannot be considered as over one-half the efficiency of the ordinary system. The only advantage of the coil system of jacketing is the mechanical one of acting as bands or hoops around the cylinder, and thus increasing its resistance against internal pressure; but for the proper purpose of a steam jacket, which is the prevention of cylinder condensation, it is greatly inferior to the ordinary system. The water of condensation from the steam jackets, being at nearly the boiler temperature, was discharged into the condenser instead of into the boiler, and so lost nearly the heat due to the difference between the condenser and boiler temperatures.

Boiler.

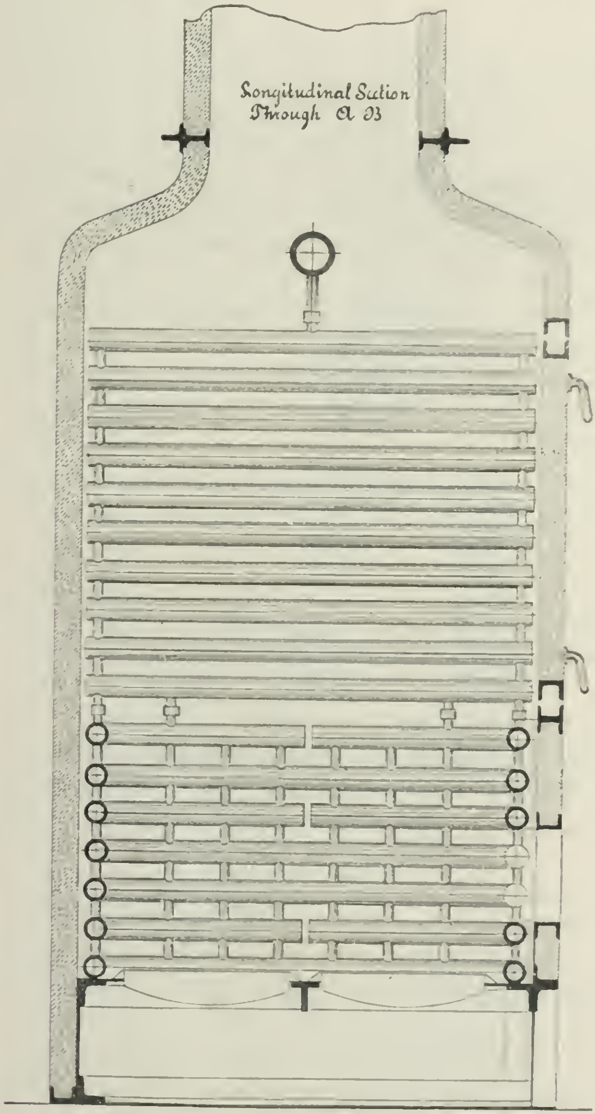
There is one boiler, and it is composed entirely of water tubes enclosed in a double shell of thin plate iron, the intervening space of which is filled with vegetable black as a non-conductor of heat. The entire thickness of the shell is 4 inches. About one-half of the tube

surface is steam generating surface, and the remaining half is steam superheating surface.



The boiler has one furnace, the grate of which is horizontal, forming a parallelogram $45\frac{3}{4}$ inches wide and 49 inches long; the corners of this parallelogram are rounded on radii of $6\frac{1}{2}$ inches.

All the steam generating tubes are water tubes, that is to say, they contain the water to be converted into steam, and they are completely surrounded by the hot gases of combustion. All the superheating



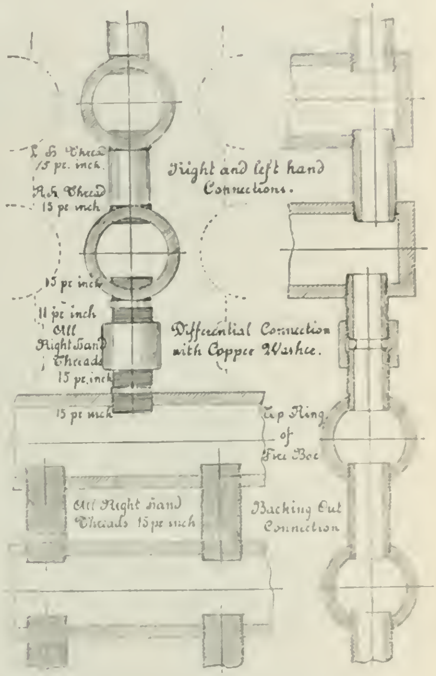
tubes contain steam and are completely surrounded by the hot gases of combustion. All the tubes, both steam generating and steam super-

heating, are of iron, 3 inches in outside diameter and $2\frac{1}{4}$ inches in inside diameter, the thickness of the metal being $\frac{3}{8}$ inch.

The grate is enclosed by horizontal tubes, seven in number, arranged one immediately over the other so as to form the sides of the furnace. These tubes are separated vertically by spaces $1\frac{3}{4}$ inches high in the clear. Each of these tubes is bent to the form of the grate which it just encloses, and the ends of the tubes are closed. The opening for the furnace door is made by allowing its width between the ends of two tubes, the height being composed of two tubes and three intervening spaces, that is $11\frac{1}{4}$ inches. The ends of each of the remaining five tubes are separated by a space of $\frac{1}{2}$ inch, otherwise the two ends of the same tube would have abutted. The seven horizontal tubes forming the sides of the furnace are connected together at intervals of about 8 inches by vertical pipes $1\frac{5}{16}$ inches in outside diameter and $\frac{7}{8}$ inch in inside diameter; the extreme length of each of these short vertical pipes is $2\frac{1}{12}$ inches, including the portions screwed into the two horizontal tubes it connects. These short vertical pipes are secured into the horizontal tubes with right and left hand threads, being screwed simultaneously into the upper and lower horizontal tubes they connect.

Above the seven tubes just described as forming the sides of the furnace, and extending over the grate, are one hundred and forty tubes, each 55 inches in extreme length and having both ends closed. These tubes are all horizontal and parallel. They are arranged ten tubes vertically and fourteen tubes horizontally. The vertical spaces between the tubes are $1\frac{3}{4}$ inches high, and the horizontal spaces between them are $\frac{3}{4}$ inch wide. The tubes of each of the fourteen vertical rows are placed over each other, their axes being in the same vertical plane, and they are connected together at each end by a short vertical pipe of the same dimensions described for the connections of the seven horizontal tubes forming the sides of the furnace. Between the ends there are no connections, nor are the tubes of the different vertical rows connected horizontally with each other. Each vertical row of ten tubes thus forms a distinct section of the boiler. The lower tube of each vertical row is connected with the upper of the seven horizontal tubes forming the sides of the boiler in the same manner as with the tube above it, namely, by a short vertical pipe at each end. The axes of the corresponding tubes of the fourteen vertical rows are in the same horizontal plane.

Above the tubes just described, and with its axis at right angles to their axes, is a steam collector composed of an iron pipe 51¾ inches in extreme length, 5 inches in outside diameter and 4 inches in inside diameter, thickness of metal ½ inch. This pipe is placed at the centre of the tubes with its axis 11 inches above the axes of the tubes of the upper row. It is connected with the tubes of each of the ten vertical rows by a vertical pipe at the centre of their length $1\frac{5}{16}$ in. in outside and ⅞ inch in inside diameter. The steam pipe leading to the engine, 1⅛ inches in inside diameter, proceeds from the steam collector.



The whole of the tubes, both those surrounding the grate and those lying above it, including the steam collecting pipe, are surrounded entirely by the hot gases of combustion, the water and steam being contained within them.

Above the steam collector the shell of the boiler is arched over, and at its centre the chimney is placed. The shell is extended below the grate sufficiently to form a closed ash pit which is provided with a door.

Large hinged doors are constructed in the shell of the boiler above the furnace door. These doors uncover, when opened, one end of each of the one hundred and forty tubes, so that they can be swept easily of soot and ash from the outside, the tubes lying in the direction of the grate bars and at right angles to the doors for sweeping.

There is no fixed water level. It can be carried anywhere at will. All the tube and pipe surface below it is steam generating surface, and all the tube and pipe surface above it, including the steam collector, is steam superheating surface. During the experiment made at the New

York navy yard with the machinery of the *Anthracite*, the water-level in the boiler was carried steadily at the top of the fourth horizontal row of the one hundred and forty tubes above the furnace. That is to say, the entire surfaces of this row of tubes and of all those below it were water heating surfaces, and the entire surfaces of all those above it, including the steam collector, were steam heating surfaces. The areas of water heating and steam heating surfaces about to be given were calculated according to this water level, as well as the steam room and water room.

Length extreme of the boiler,		5 feet 3 in.
Breadth extreme of the boiler,		4 feet $10\frac{1}{2}$ in.
Height of the boiler,		9 feet 2 in.
Area of grate surface,		15·3158 sq. ft.
Area of water heating surface, calculated for the exterior circumference of the tubes,		300·24 sq. ft.
Area of water heating surface, calculated for the interior circumference of the tubes,		225·49 sq. ft.
Area of steam superheating surface, calculated for the exterior surface of the tubes,		325·89 sq. ft.
Area of steam superheating surface, calculated for the interior surface of the tubes,		239·20 sq. ft.
Cross area for draught between the tubes,		3·724 sq. ft.
Steam room,		11·048 cu. ft.
Water room,		10·109 cu. ft.
By Calculation.	Weight of tubes and their connections in boiler,	8264 pounds.
	Weight of the grate bars,	728 pounds.
	Weight of the ash pit,	186 pounds.
	Weight of the boiler base or bearer,	380 pounds.
	Weight of the chimney,	280 pounds.
	Weight of the metal in the boiler casing,	1804 pounds.
	Weight of vegetable black filling for the casing,	3750 pounds.
	Total weight of boiler, exclusive of water,	15392 pounds.
	Weight of water at 62 degrees Fahrenheit in boiler,	630 pounds.
	Total weight of boiler and its contained water,	16020 pounds.
Square feet of water heating surface, calculated for exterior surface of the tubes, per square foot of grate surface,		19·6033

Square feet of water heating surface, calculated for interior surface of the tubes, per square foot of grate surface, ...	14·7227
Square feet of steam superheating surface, calculated for exterior surface of the tubes, per square foot of grate surface, ...	21·2178
Square feet of steam superheating surface, calculated for interior surface of the tubes, per square foot of grate surface, ...	15·6179
Square feet of grate surface per square foot of cross area for draught between the tubes, . .	4·1127

(To be continued.)

Crystal Palace at St. Cloud.—Plans have been prepared for a large crystal palace, to be built in the park of St. Cloud, for permanent exhibitions of industry, art, horticulture, scientific spectacles with experiments upon a large scale, together with pictures and representations of the vegetable and animal kingdom in different geological ages. There will also be views and models of ancient and modern monuments, and curiosities from all parts of the world. It is proposed to combine the attractions of the Sydenham palace, the Kensington museum and the Richmond greenhouses.—*Les Mondes.* C.

Scholastic Thermo-dynamics.—M. A. Vacant claims that nearly all the postulates of thermo-dynamics were held by the schoolmen, who taught the following facts: 1. Heat, light, electricity, weight are accidental properties of bodies and not material fluids. 2. The forces capable of producing physical phenomena may be either latent or sensible. 3. Latent forces do not undergo any transformation. 4. Sensible forces are either transformed or transmitted. 5. Forces are only transmitted in tendencies to restore equilibrium. 6. All physical forces can be transformed so as to be reproduced, according to circumstances, sometimes under the form of latent work, sometimes in that of local movement, of sound, heat, light, magnetism, electricity, weight or expansion. 7. No new force is acquired and none is lost in transformation or transmission. 8. The sum of work and of sensible movement never varies in our universe. 9. Local movement is the cause of all physical phenomena—*Rev. des Sci. Eccles.* C.

The DETERMINATION of SILICON and TITANIUM in PIG IRON AND STEEL.

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Read at the Meeting of the American Institute of Mining Engineers, February, 1880.

In a communication to this Institute at the Baltimore meeting, February, 1879,* on the "Determination of Silicon in Pig Iron and Steel," the method recommended was the treatment of the metal with nitric acid until action had ceased, and then evaporating with sulphuric acid until the nitric acid was nearly or quite driven off. After filtration of the siliceous and carbonaceous residue, and washing with hot water and hydrochloric acid, a silica was obtained, on ignition, which was quite pure. Since this paper was read before the Institute, we have had large experience with the method, and find it uniformly reliable. Results are obtained in a few hours with the greatest accuracy.

While testing the method and comparing it with others in general use, and also with some new methods, there have been developed some facts which may be of sufficient value to lay before the members of the Institute.

Interesting results were obtained by the treatment of iron borings in a platinum crucible with acid potassium sulphate at a red heat. The operation must be conducted with care, to prevent too violent action; but a little practice will enable one to effect the complete oxidation of one gram of iron (the amount usually taken) in from 20 to 30 minutes. On subsequent solution of the fused mass in water, a little hydrochloric acid is added to dissolve any ferric oxide which may adhere to the crucible. For 1 gram of iron, about 25 grams of the acid potassium sulphate are used. This amount is ordinarily added at once to the iron in the crucible. The operation must, of course, be carefully watched that the mass does not flow over the top. It should not mount higher than three-fourths of the height of the crucible, which should have a capacity of not less than 70 cc. If the operation has been successful, a nearly white mass will remain in the crucible,

* Transactions, vol. vii, p. 346.

without a particle of graphite. The mass may be poured out while liquid, but a more convenient method is to insert into the fluid mass a piece of heavy platinum wire, bent at the end, and then allow the mass to solidify around it. The crucible is then slightly warmed to loosen the contents, which can be lifted out by the wire. The fused mass, with the crucible and lid, is put at once into boiling water with some hydrochloric acid. When solution is complete, the silica is filtered off and washed with hot dilute hydrochloric acid and water. After drying, the filter, with its contents, is ignited and weighed. The resulting product should be pure white. While accurate results have been obtained by this method in 45 minutes, yet a long experience with it shows that it is not to be relied on for all kinds of iron and steel. The following are some of the results obtained:

	1	2	3	4	5	6	7	8	9
Silicon by nitric and sulphuric acids....................	0·737	0·772	1·24	1·214	2·35	2·40	2·40	0·755	0·622
	0·739	0·777	1·25		2·37	2·42	2·40	0·757	0·629
Silicon by fusion with acid potassium sulphate.........	0·628	0·772	1·41		2·37	2·53	2·46	0·749	0·535
	0·677	0·702		1·250			2·47	0·761	0·566
		0·677					2·47		0·575
							2·49		0·591
							2·50		
							2·50		
							2·52		
							2·52		
							2·54		
							2·58		
							2·69		

	10	11	12	13	14	15	16	17	18
Silicon by nitric and sulphuric acids....................	1·92	0·207	4·40	0·165	0·929	3·75	0·027	0·682	0·205
		0·208	4·39	0·166			0·027	0·663	0·207
		0·210							
Silicon by fusion with acid potassium sulphate.........	1·94	0·093	4·60	0·116	0·929	3·91	0·000	0·529	0·065
	1·94	0·141		0·130		1·01	0·025	0·563	0·115
		0·231				4·02		0·598	0·120
		0·287				4·19		0·628	0·197
								0·661	0·209

1. Richmond warm-blast charcoal iron, No. 3. 2. Greenwood cold-blast charcoal, No. 1. 3. Dutchess, anthracite, No. 1. 4. Hecla cold-blast charcoal, No. 2. 5. Bushong, anthracite, No. 1. 6. Leesport, anthracite, No. 1. 7. South Easton, anthracite, No. 1. 8. Glendon, gray forge. 9. Glendon, mottled. 10. Durham, anthracite. 11. White iron. 12. Silver-gray iron. 13. Spiegeleisen. 14. Source unknown. 15. Source unknown. 16. Bessemer steel. 17. Bessemer steel. 18. Sanderson tool steel.

In the above table will be noticed many results which vary greatly from the true percentage, and for which variation no sufficient explanation is at hand. In general, it may be said that irons high in silicon give better results than those low in silicon. With silicon over one per cent., the tendency is toward too high results; with silicon under one per cent. the tendency is toward low results. When the silicon is about one-half of one per cent. or lower, the results are, moreover, very uncertain, as will be seen from the figures for mottled and white iron, also for spiegeleisen and steel. In one experiment on a sample of Bessemer steel (No. 16), no silicon was found, while, in another experiment with the same steel, made by completely driving off the free sulphuric acid from the acid sulphate, and then adding a fresh portion, the percentage of silicon obtained agreed with that by nitric and sulphuric acids. For iron or steel very low in silicon this last procedure is necessary to get even approximate results; but for ordinary pig irons, it gave no better results than were obtained by simply heating the borings with acid potassium sulphate until all traces of graphite had disappeared. Silver-gray iron is with difficulty oxidized by this method, although the results obtained from one sample were reasonably good.

For Bessemer works, where a rapid method for the determination of silicon is often desirable, this method will perhaps find a useful application. It should be mentioned that we have found great difficulty in buying acid potassium sulphate free from silica or other insoluble matter. In all cases we found it necessary to purify the sulphate by solution in water, filtration, evaporation and fusion.

Some variations were tried on the method. The pig iron was first oxidized in the crucible by nitric acid and the resulting product treated with the acid sulphate. Again, nitre was used in connection with the acid sulphate. In another series of experiments the iron was heated to redness for some time with sodium carbonate (which has the effect of oxidizing energetically the carbon and silicon),* and subsequently treated with sulphuric acid and acid sulphate. These variations were not accompanied with any better results than when the acid sulphate was alone used.

The high results are mostly caused by oxide of iron, which attaches itself in small amount to the upper part of the crucible, and which is somewhat slow of solution in acid. It does not follow that silica which is quite white after ignition is free from iron.

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*Transactions, vol. vii, p. 146.

The facility with which pig iron and steel can be brought into complete solution by fusion with acid potassium sulphate will perhaps recommend this procedure when other ingredients besides silicon are to be determined.

In comparing the silicon results obtained by the nitric and sulphuric acid process with those obtained by the use of hydrochloric acid, we noticed that the results by the latter process were almost invariably higher when the residual silica obtained after burning off the carbon was not refused with alkaline carbonates. The same is true when sulphuric acid is used alone without nitric.

In many cases the silica was found to contain iron oxide or other bases, but the higher results were also obtained when the silica was found to be free from metallic oxides. Investigations showed the presence of titanic acid, and an extended series of experiments has shown that titanium is very generally present in pig iron.

In determining the titanium, Riley's method was generally used, which consists in treating the pig iron with hydrochloric acid and filtering off the siliceous graphitic residue, which is, after ignition, fused with acid potassium sulphate. This method gives fair results, but a more accurate method we found to be the treatment of pig iron in a porcelain boat in a glass tube with dry chlorine at a red heat. Pig iron thus treated is almost completely volatilized, a small carbonaceous residue—five per cent. or less—remaining in the boat. The ferric chloride, with some manganic chloride, condenses in the glass tube (which should be long enough to allow of this), and the non-metals are driven over as gaseous chlorides.

For the absorption of the silicon and titanium a series of three or four tubes or bottles of water is used. No precipitate is noticed in the water, but, on boiling, titanic acid contaminated with silica is precipitated. To determine the silica and titanic acid, the contents of the bottles are poured into an evaporating dish and strongly acidified with hydrochloric acid. Fifteen cubic centimetres of sulphuric acid (sp. gr. 1·23) are added, and the solution evaporated until all the hydrochloric acid is expelled. The silica is thus rendered insoluble and the titanic acid retained in solution, from which it can be precipitated after dilution by boiling. The results by this method are always a little higher than those obtained by Riley's method. In the treatment of pig iron by nitric and sulphuric acids, the silica obtained is free from titanic acid, which goes entirely into the filtrate. It is not possible,

however, to get more than about one-third of the total amount by precipitation by boiling, owing, doubtless, to the presence of the relatively large amount of iron in solution.

The following table shows the relation between the silicon and titanium in a few pig irons containing notable quantities of titanium:

	Glendon gray forge.	Silver-gray.	Source unknown.	Source unknown.	Leesport.	Bushong.
Titanium by Riley's method	0·099	0·114	......	0·318	0·115	0·225
" " chlorine "	......	0·278	0·216	0·374	......	......
" calculated as silicon*	0·077	0·217	0·170	0·291	0·081	0·173
Sum of last with true percentage of silicon............	0·832	4·607	1·460	1·751	2·481	2·523
Silicon by HCl method, without refusing............	0·811 / 0·815	4·650	1·640	1·840	2·520 / 2·550	2·590 / 2·580
True percentage of silicon...	0·755	4·390	1·290	1·460	2·400	2·350

Other determinations of titanium in pig iron by Riley's method are as follows:

	Per cent. of Titanium.
Richmond, warm-blast, charcoal, No. 3,	0·018
Greenwood, cold-blast, charcoal, No. 1,	0·052
Hecla, cold-blast, charcoal, No. 2,	0·048
Dutchess, anthracite, No. 1,	0·055
Leesport, anthracite, No. 1,	0·115

A few more details of the treatment of pig iron with dry chlorine may be worth giving in the accompanying tabular form:

Pig Iron Treated.	Total percentage of residue in boat.	Siliceous residue after burning off carbon.	Percentage of carbon thus determined.	Silicon determined in this residue.	Silicon from solution in water.	Total silicon by Cl method.	Total silicon by HNO_3 and H_2SO_4 process.
South Easton...........	3·92	0·292	3·628	0·031	2·335	2·366	2·400
Glendon Gray Forge.	4·17	0·200	3·970	0·022	......	......	0·756
Glendon Mottled	4·29	0·302	3·988	0·015	0·444	0·459	0·622
" "	4·15	0·266	3.884	......	......	......	0·622
Source unknown (1)..	4·53	0·226	4·304	0·032	0·873	0·905	0·970
" "	4·64	0·277	4·363	0·043	0·908	0·951	......
Source unknown (2)..	4·75	0·415	4·335	0·083	1·260	1·343	1·460
" "	4·74	......	......	0·080	1·340	1·420	......
" "	5·22	......	......	0·080	1·330	1·410	......
" "	4·83	0·483	4·497	0·080	1·300	1·380	......
White Iron...............	4·04	0·224	3·816	0·080	0·152	0·232	0·209
Silver-gray...............	3·38	0·240	3·140	0·045	4·090	4·135	4·400

* That is, the amount of silicon which would be calculated from the titanic acid mixed with the silica resulting from the hydrochloric acid treatment.

It will be seen from the above that the silicon is fairly accounted for in nearly all instances. A more thorough absorption of the silicon chloride by water, or, perhaps still better, by an alkaline solution, may give the full amount of silicon. As far as experiments go, there is no silicon with the condensed ferric chloride in the tube. Phosphorus is present in the ferric chloride, and sulphur is present as sulphuric acid in the water used for absorption, but we have not yet followed up these elements.

When the carbonaceous and siliceous residue in the boat is treated with water, a portion goes into solution, and in this solution may be detected, besides manganese, which we might expect, aluminum, magnesium and calcium. Whence come these latter metals? Were they present in combination with the iron, or do they simply indicate the presence of cinder in the iron?

In the portion of the residue insoluble in water these elements are likewise found, and it may be that the soluble calcium, magnesium, etc., were present alloyed with the iron, and the insoluble compounds of these metals were in the cinder. More experiments are needed to clear up this doubt.

In an experiment bearing on this point dry chlorine was passed at a red heat over cinder which had not been more than 24 hours out of the furnace, and it was found to increase in weight about 3 per cent., showing that the absorption of chlorine was not very marked. In a specimen of old cinder, the gain in weight under the same conditions was from 16 to 19 per cent. The action seemed to be the conversion of carbonates of the alkaline earths into chlorides.

When dry chlorine is passed over a mixture of a titaniferous ore and charcoal at a low red heat, titanium chloride is volatilized; but when a mixture of a blast-furnace cinder and charcoal is similarly treated, no silicon chloride is formed. It is possible, therefore, that the silicon remaining in the boat after the treatment of pig iron by chlorine may result from the presence of cinder in the iron. More experiments are needed before any decided assertion can be made on this point.

It was expected that the treatment of pig iron by chlorine at a low red heat would give a separation of iron from manganese. We were unsuccessful in effecting this separation. In all cases, some manganese was found with the ferric chloride. With spiegeleisen, or ferro-

manganese, the fusion of the manganic chloride in the boat rendered it difficult to volatilize all the iron.

In the paper previously alluded to on the determination of silicon, it was stated that in the treatment of pig iron by hydrochloric acid about one-third of the silicon was found in solution and two-thirds in the residue. Further experiments have shown that the relative amounts of silicon in solution and in the residue depend on the strength of the hydrochloric acid. Thus, in an iron containing 0·738 per cent. of silicon we found in the insoluble residue, after treating with hydrochloric acid, as follows:

With acid of sp. gr. 1·20, . .	0·616
" " 1·12, . .	0·440
" " 1·015, . .	0·006

Again in an iron with 2·36 per cent. of silicon we found:

With acid of sp. gr. 1·20, . .	2·26
" " 1·12, . .	2·05
" " 1·015, . .	0·02

There is no loss of silicon by volatilization in treating gray or white iron with hydrochloric acid.

Influence of Temperature on Tuning Forks.—Kayser finds that the number of vibrations of a tuning fork between 9° and 30° (32° and 86° F.) is a linear function of the temperature; the influence of temperature increases with the sharpness of the note, the variation for one degree being sensibly proportional to the square root of the number of vibrations. Within the limits named the coefficient of atmospheric elasticity increases with the temperature.—*Les Mondes.* C.

Uniformity of Vegetable Composition.—H. Pellet has continued his investigation upon vegetable composition by analyzing numerous specimens of potatoes. He finds that there is a constant ratio between the total amount of phosphoric acid contained in the entire plant and the starch; there is a similar ratio between the starch and the mineral substances which are absorbed, exclusive of silica; there are great differences in the proportions of the principal alkalies, but the quantity of sulphuric acid necessary to saturate all the bases is sensibly the same.—*Comptes Rendus.* C.

AN ADAPTATION OF BESSEMER PLANT TO THE BASIC PROCESS.

By A. L. HOLLEY, Memb. Inst. Civ. Eng., etc.

THE BASIC PROCESS.

The process of dephosphorizing iron in the Bessemer converter, held in this country under patents of Thomas, Snelus and Riley, is called, by common consent, the Basic Process.

In the ordinary Bessemer operation, when the flame "drops," as observed by the naked eye, or when the carbon lines disappear, as more accurately observed through the spectroscope, there still remain in the metal some hundredths of a per cent. of carbon, also of silicon, also all the phosphorus which the metal originally contained. Further blowing would oxidize the iron itself. But if the slag in the converter, instead of being acid (chiefly silica), as in the ordinary operation, is basic (chiefly lime), a small part of the phosphorus will be found in the slag, instead of in the metal, when the flame drops. And if the blowing is further continued for two or three minutes, all the phosphorus, excepting a few hundredths, will be found in the slag, and the iron will not have been much oxidized; it will have been protected by the phosphorus. Chemists disagree as to the precise reactions which occur; we suppose that phosphorus is always oxidized by the air blast, and that it constantly returns to the iron, in presence of an acid slag; we know that a basic slag retains the phosphorus, however it may have got it.

The basic process, therefore, consists of two things: First, the maintenance of a basic slag; second, the "afterblow."

First. The basic slag is *formed* by the addition of about twenty per cent. of lime to the iron charge in the converter before or during the blowing. The basic slag is *maintained* chiefly by making the converter lining of lime, and also by using iron low in silicon. An acid lining would be destroyed by the lime additions, and would vitiate the slag. The latter result would be produced also by silica formed by the oxidation of the silicon in the iron. The grand difficulty has been the limited durability of the basic lining. After much costly experimenting, practicable linings have been made of dolomite brick-

—bricks formed by wetting and moulding pulverized magnesian limestone and then burned at the highest attainable temperature. The magnesia prevents the bricks from crumbling when exposed to the air. Linings are also formed by ramming hard-burned, pulverized dolomite, mixed with ten per cent. of tar, into the converter. Ordinary fire-brick tuyeres are used in lime bottoms, or the bottoms are rammed around rods which form tuyere-holes.

Second. The afterblow presents little difficulty; its duration is soon determined for any grade of material and products.

In order that the phosphorus may be thoroughly removed—it seems rather paradoxical to say—there must be plenty of it. Silicon, the chief heat-giver in the ordinary process, must, as we have seen, be kept low. Phosphorus is also a heat-giver, but there must be enough of it to maintain, by its combustion, perfect fluidity in nearly pure iron. At a lower temperature the Bessemer process could not be completed. Iron best adapted to the basic process has two to two and one-half per cent. of phosphorus and under one per cent. of silicon, also one and one-half to two and one-half per cent. of manganese—a heat-giver and a valuable ingredient in steel—but the manganese may be dispensed with at this stage of the operation. The afterblow completely removes silicon and reduces other impurities.

Spiegeleisen, or ferro-manganese, are added to the blown metal, but most of the slag is first poured out of the converter, so that the manganese shall not carry the phosphorus out of the slag to the iron. Otherwise the process is conducted in the usual manner.

In nearly all parts of the United States there are phosphoric ores adapted to the basic process. They are usually cheap, and in some regions, of the South especially, they are so abundant, and so associated with coal and limestone, that the manufacture of cheap steel is likely to become, in such localities, a vast and important industry.

The maintenance of refractory linings in Bessemer converters, in such a way as to promote regular and maximum production, has been the subject of more experimenting than any other feature of the Bessemer system, and it is still the least perfect and satisfactory feature, excepting perhaps the casting of steel. Linings are not only eroded by the mechanical action of the charge, but they are chemically decomposed by its various slags. The silica linings usually employed have, indeed, been so improved that an average of say sixty charges per twenty-four hours can be got out of a pair of converters, and the

shifting of interchangeable converter bottoms (containing the tuyeres) is so rapid that it does not delay production: but the repairing of the fixed lining just above the tuyeres, where both mechanical and chemical action are most severe, is frequently the cause of delay, and the operation rapidly performed between heats is tedious and costly. The accumulations of slag on other parts of the lining must also be quarried out, else the converter will become too small for the charge.

These are the conditions of maintaining silica linings: but the difficulties are increased, probably about threefold, when the linings are made of lime, for the basic process. The basic process consists in removing phosphorus from the iron under treatment by retaining the phosphorus oxidized by the blast, in a basic slag formed of say twenty per cent. of lime added to the charge. An acid (silica) lining would vitiate the basic slag, and would also be rapidly destroyed by it. Lime containing some magnesia, and produced by burning magnesian limestone (dolomite), is at present the only basic material successfully used for converter linings. It is usually made into bricks, which are hard burned and built up with mortar of similar material to form the lining.

Basic bottoms and tuyeres stand ten to fifteen charges, nearly equaling acid bottoms, and they may be readily changed; but basic linings, near the tuyeres, and also in other parts where abrasion is severe, wear rapidly and must be frequently repaired by cooling the converter and inserting new bricks, or patching in some suitable manner. The converter is thus put out of use for at least twenty-four hours—a very serious delay to production. From a wide observation, the author feels safe in saying that a basic lining is rarely run above sixty charges without extensive repairs, and in some works repairs are made every time a bottom is set. With some irons there is also an accumulation of slag around the mouth of the converter; its removal sometimes also causes further delay.

The output of a pair of converters in Europe averages about half that of a pair of converters of the same size in the United States, and is often less than half. The limited endurance of basic linings in Europe is, therefore, a less conspicuous defect than it is here, where one converter must make 25 or 30 charges in twenty-four hours, so that the repairs of basic linings, as at present conducted, would keep an American plant idle half the time. This delay is really as impor-

tant in Europe as it is here; the greater the output from a given plant, the cheaper the product.*

In order, therefore, that the basic process may come into extensive use, basic linings must be so maintained that their output will nearly equal that of acid linings.

There are two reasonable conditions of improvement: the one is to prolong the endurance of basic materials, so that their repairs can be made with little delay, while the converter is in position for use. There seems to be little or no progress, or probability of immediate progress in this direction. The other is the rapid and complete removal of a worn lining and the replacement of a repaired one. A third system, seriously proposed, is to double or treble the entire converting plant. The only practicable way to replace a refractory lining (which cannot be handled by itself) is to replace the vessel which contains it. The worn portions of the lining may thus be repaired at leisure, in another part of the works, rather than in position for use, where repairs would retard output.

An obvious way to replace an entire converter lining is to replace the entire converter. This system is already under construction in Europe. The method is also obvious—lifting the converter bodily out of its pillow blocks, and conveying it to the repair shed by means of an overhead traveler; then setting a repaired converter in place by the same means. Such a plant is doubtless cheaper than a duplicate plant, and its output should be materially greater than that of fixed converters. But the operation of changing an entire converter must be slow and tedious. When the arrangement is such that pillow block caps are required, these must be loosened by unscrewing heavy nuts; then they must be made fast to the crane chain, lifted, traversed and set down. The blast pipe connection must be broken, and possibly some platforms must be removed. Then the traveler is placed exactly centrally over the converter, ponderous chains are made fast, the mass is raised high enough to clear surrounding parts, and drawn laterally to the repair shed; then the converter is placed centrally over its seat and lowered and steadied (as it swings from a chain) into its pillow

* The statement sometimes made in England that the rapid production in America impairs quality of product is but a cover for inadequate plant. Steel is obviously no better because five hours instead of one are consumed in setting a vessel bottom, or because it may take twice as long in an English works to handle materials and product.

blocks. The repaired converter is raised, traversed and set in place by repeating all these operations; the blast connection is then made, and the pillow block caps are lifted, traversed, steadied into place and screwed down. If the converter is removed in sections, transferring each section and making the refractory joints will occupy much more time. The chimneys and the openings in the side of the building must be high enough to make passage not only for the traveler but for the converter when lifted out of its seat, and for the chains that sustain it. A traveler of the required power, height and length is obviously a ponderous and costly structure, and to work with reasonable speed it must have independent steam power—the hydraulic system of the works cannot well reach it.

The method of replacing the lining proposed by the author, and shown in the engravings, is *removing only the shell* of the converter: lowering it out of the trunnion ring easily and rapidly, by means of a simple lift and car, and replacing a repaired shell by the same means. No pillow block caps, blast connections, nor other surrounding parts are touched; a dozen cotters are knocked out, the shell is lowered and run straight back to the repair shed, the new shell is run in, lifted and cottered on; this is all. The machinery and transference are on the general level, and not forty feet or more up in the air. The car may be moved by a small reversing engine or by a hydraulic capstan, by means of a wire rope and sheaves suitably arranged. The car runs against a stop, and the lift is perfectly vertical, so that the shell may be put in place by two rapid motions without the delay of adjustment.

The lining may be heated before the shell is put in place, and bottoms (and tuyeres) may be separately removed, as at present, or they may be taken away with the shell and repaired without removal from it. In the latter case, the shell must be placed in trunnions, in the repair shed, so that the bottom may be turned downward for repairs. But if the bottom is first removed, the shell need not be placed in trunnions in the repair shed; the shell will stand mouth downward on the car, a position most favorable for repairing both the mouth and the lining about the tuyeres, which are the two places chiefly needing repairs. This is doubtless the better plan, and it saves the cost of supplementary trunnion rings and turning gear. The engravings show the converter hung so high above the general level that the bottom and tuyere box can be hauled out, with the shell, under the trunnion ring. In case

the bottom is previously removed, the converter may be hung some three feet lower.

It has been remarked that in American works converter bottoms are changed so rapidly that one is always ready, even when tuyeres stand but eight or ten operations. Changing converter shells is much more rapid than changing bottoms. The several operations of removal and transportation are the same, but the converter lining must be trimmed out to receive the new bottom, and a refractory joint must be made. The new shell has merely to be cottered on.

The comparative cheapness of apparatus to change the shell, instead of the entire converter, is obvious. The two hydraulic lifts for removing the bottoms are made heavier, and there are several cars of simple construction; this is the entire extra apparatus. The increased cost of the converters is not important. In the other case, the traveler with its engine, and the standards and turning gear in the repair shed, and the trunnion rings and pinions (the chief cost of the converters) for each spare shell, approach in expense that of a duplicate plant complete.

But one objection has been raised, as far as the author is aware, to the plan proposed, and that is the possibility of damage to the lift under the converter, in case the charge should burn through and fall upon it. To avoid such damage, the lift table may be sunk several inches below the pit level and covered with sand. It may be remarked that lifts under converters are used in nearly all the American works with satisfactory results.

The engravings illustrate the construction and arrangement so fully that little explanation is required. The trunnion ring (Figs. 1 to 4) is of cast iron, with an inch wrought iron lining; or it may better be a steel casting, which will not require a lining. There is a two-inch annular space between the trunnion ring and the converter shell, and the shell is prevented from shifting laterally by means of the wedges shown in Fig. 1. The car is raised by the lift to receive the shell; or the shell may be lowered by means of a fork on the lift passing through the car.

This construction of converters has led the way to a general improvement in the design of the plant. The shells and bottoms may be run out laterally into the converting house, but the space here is insufficient for convenient repairs, and the shells for one converter could not be well got to the other. In order that there may be one

common place for repairs, and ample room both for spare shells and spare bottoms, they must be run out in rear of the converters, as shown in Fig. 8. If blast furnace metal is brought directly to the converters this rear space is not otherwise wanted; but if cupolas are placed there, as is usually the case, they must be so arranged that the shells can pass out under them.

But the cupolas (excepting the spiegel cupolas) may best be placed elsewhere; if there are blast furnaces the cupolas may be so arranged near them as to utilize the same system of transportation, hoisting, blowing and hot blast. There should be plenty of spare gas from good furnaces to heat cupola blast. These are very important considerations, regarding both cost of plant and economy of working; and, judging from the experience at many works, the disadvantages of hauling fluid iron some thousands of feet in a railway ladle are less than those due to crowding the melting department and its stock yard and appurtenances, close behind the converters. Fluid iron is hauled from one to two miles* without chilling; it need usually be hauled but a few hundred feet, and the cost of the transporting plant and service should be about the same for the two systems. There are two important advantages in the arrangement shown by the engravings:

1. Placing only the spiegel cupolas, instead of the entire melting department, close behind the converting house leaves its rear comparatively open to free ventilation, thus cooling not only the space around the converters, but also the casting pit.

2. This arrangement provides ample room for the convenient removal of slag, which, in the basic process, is very voluminous; one long dumping car placed under both the converter and the ladle catches it all, and as the bottom of the pit is on the general level, the slag is neither handled nor lifted; the car is simply hauled out by the yard locomotive and dumped. Experts well know the cost and inconvenience of breaking up and quenching slag in the pit, and of lifting it out of the pit, and then loading and removing it.

Iron may be got to the converters in a ladle by various means. It may be hauled on the general level to one or more hoists, and run into short spouts or directly into the converter mouths, or it may be drawn up a gradual incline or lifted by a hoist to an elevated railway near the converters, and thence tipped or tapped into them directly or

* At the Barrow Works it is hauled two miles; at Ebbw Vale some five miles.

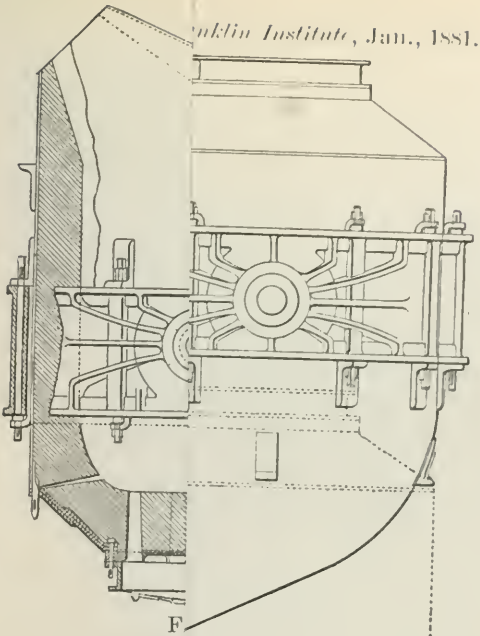
through spouts. The short elevated railway, as shown in Fig 7, has one conspicuous advantage—it is out of the way of all other apparatus and operations; it does not cross railways nor interfere with any transportation on the general level. This is an important feature when a charge is made every 20 to 30 minutes. The ladle is drawn by a locomotive to short, steep spouts leading to the converters; there is no lateral or hand movement, and hence no delay. A spout leads to each converter, chiefly for the purpose of leaving the space between the converters (where the common spout is usually placed) quite free for the spiegel ladle.

The spiegel cupolas and their appurtenances occupy so little room that they are placed, without interference with other apparatus, very near and above the converters. A railway ladle receives the spiegel from either cupola and tips it directly into the converter, quickly and hence completely, by a short run and without hoisting or lateral movement. It may be weighed in transit if desired. The wide platform between the converters is at other times free for bringing lime, scrap or other materials to the converter mouths, and these materials are conveniently raised by the cupola hoist.

The floor of the converting house is raised a few feet, so that the pit bottom may be on the general level, for the convenient removal of slag, as before explained. The ground outside of the converting house slopes gradually to the general level. This facilitates the removal of products and also the drainage.

The plant for repairing shells consists of two turn tables, some short railways and a shed; also some platforms and a lift for materials. If bottoms are to be removed with the shells there must also be mounted trunnion rings and turning gear; also a crane in the shed; but, as before explained, this seems unnecessary. Room is shown for repairing four shells at a time, but the railways may be lengthened to accommodate more. The plant for repairing bottoms consists of short railways and turn tables, a space for ramming bottoms under a shed and the necessary ovens for drying them; also a crane, which sets the bottoms directly on the oven cars. If ordinary tuyeres are used fewer ovens are required; if the bottom is all one tuyere, rammed around rods, it must be burned for two or three days, so that more and hotter ovens are necessary. The repairing department may obviously be arranged in other ways to suit special cases.

The average output of the American plant, having two 6-ton to 7-



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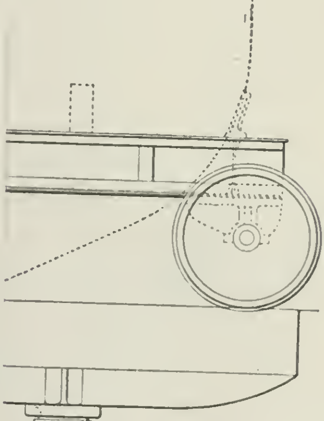
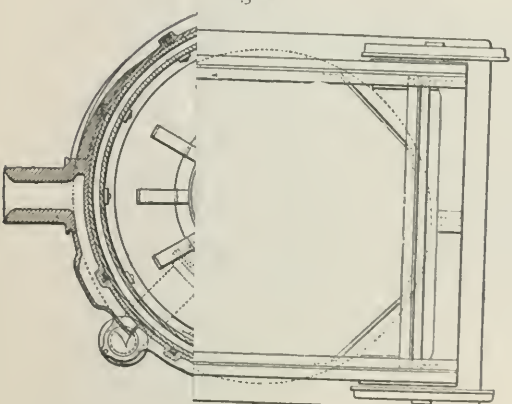


Fig. 4.



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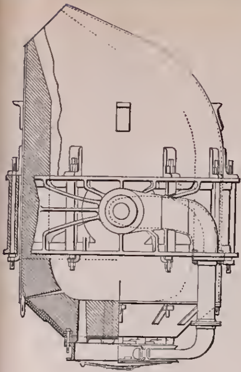

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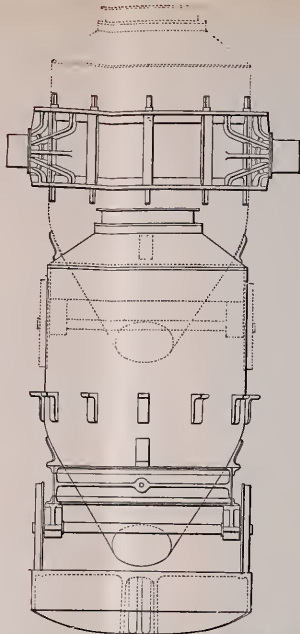


Fig. 3.

Fig. 6.

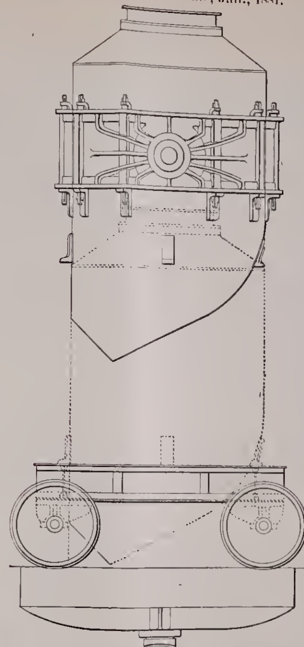


Fig. 4.

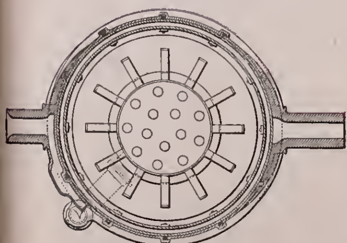


Fig. 2.

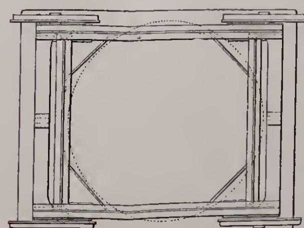
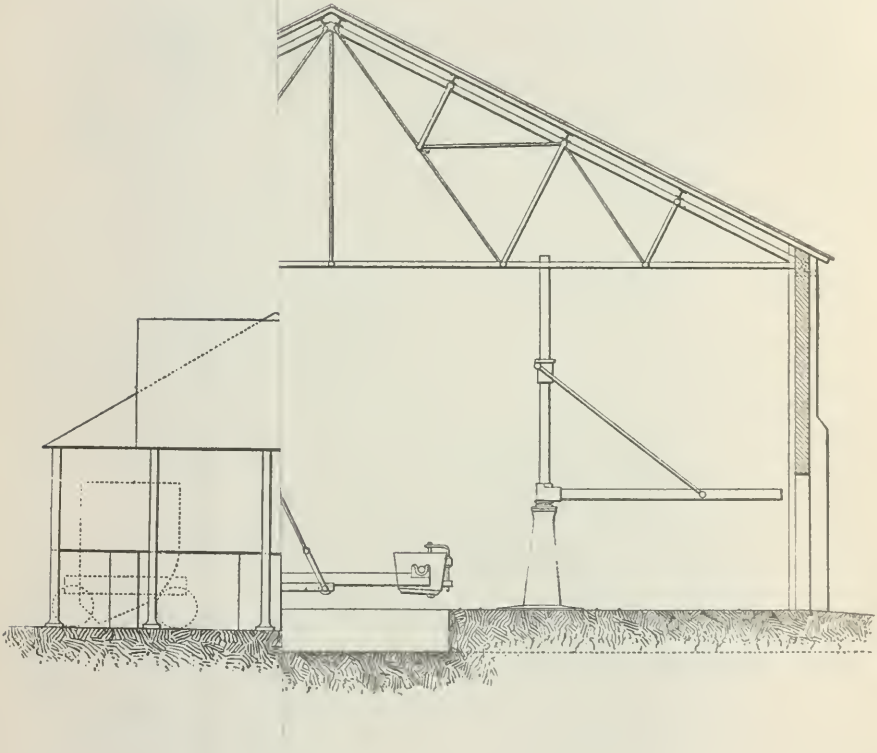


Fig. 5.

HOLLEY'S CONVERTING PLANT FOR THE BASIC PROCESS.



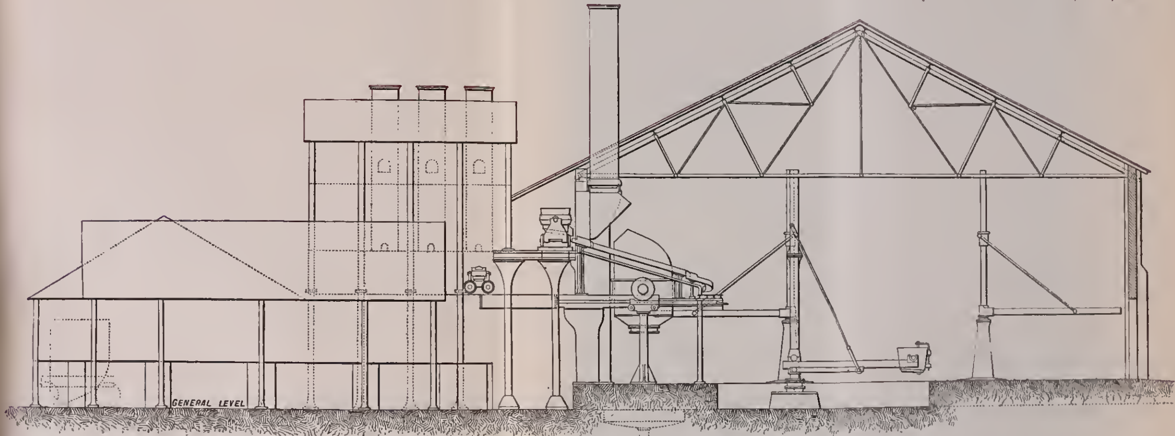
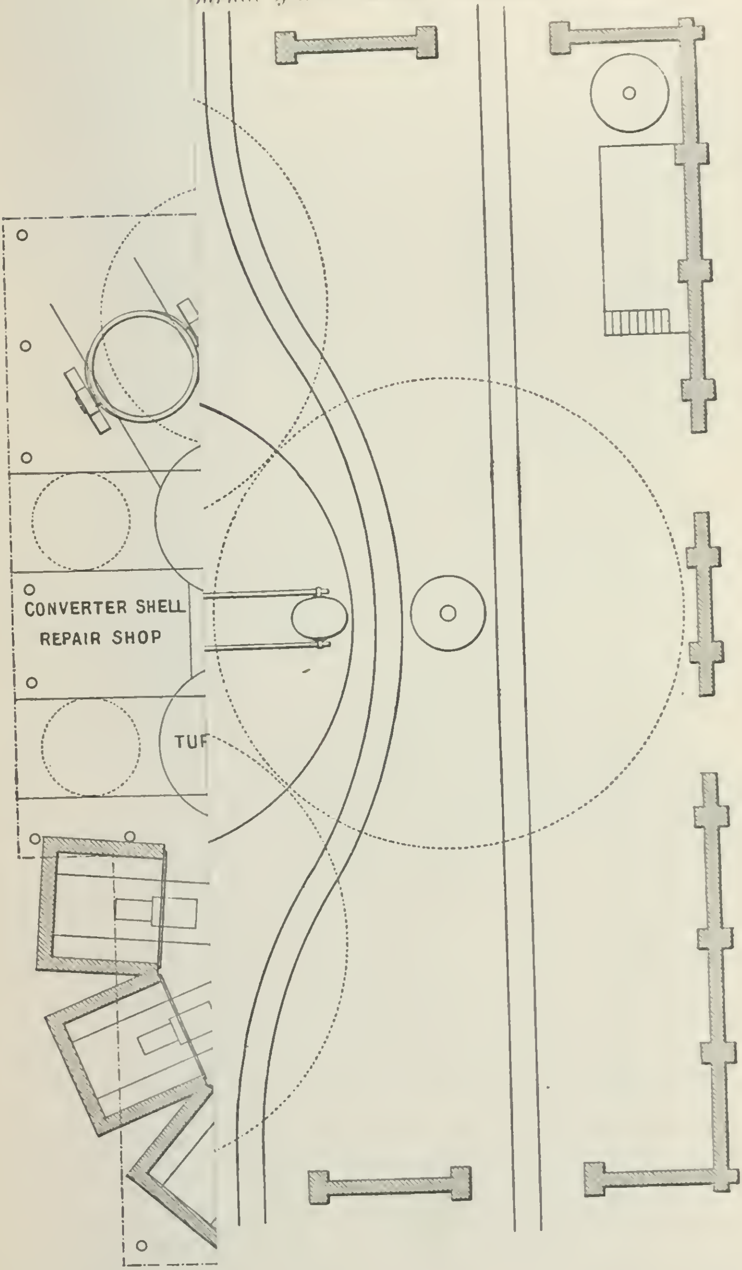


Fig. 7.
HOLLEY'S CONVERTING PLANT FOR THE BASIC PROCESS.

urnal of the Franklin Institute, Jan., 1881.



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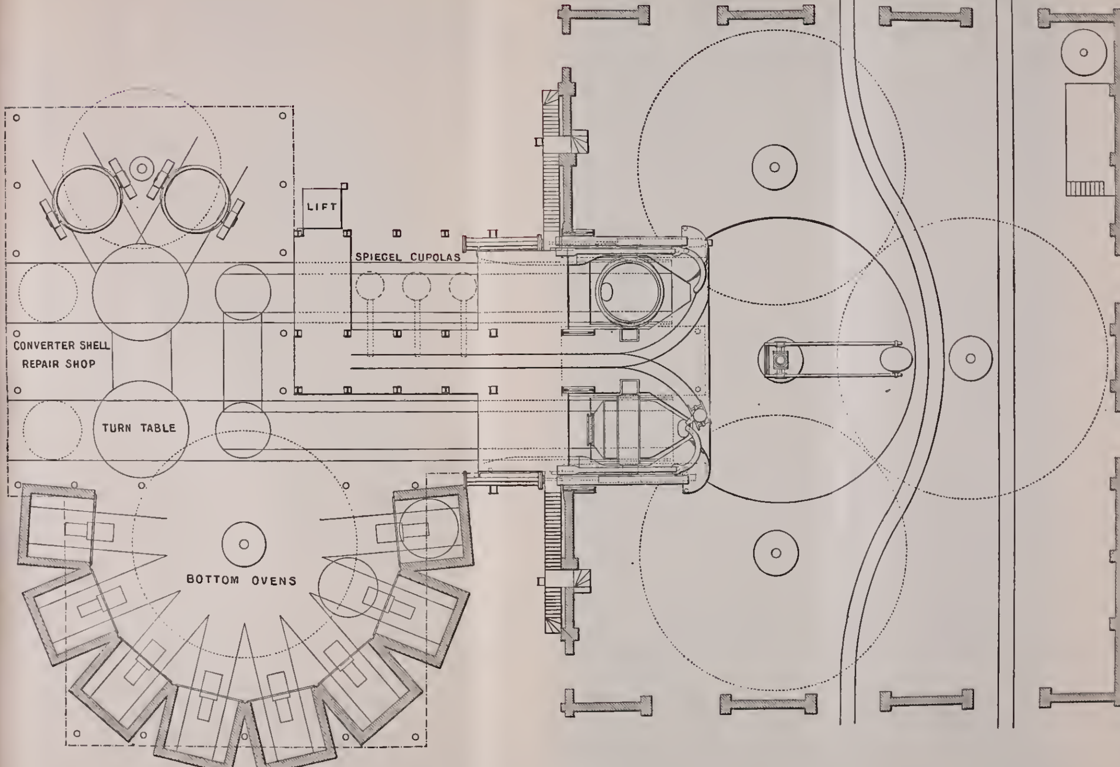

Fig. 8.
HOLLEY'S CONVERTING PLANT FOR THE BASIC PROCESS.

ton silica lined converters in one pit, is 100,000 tons of ingots per year. It will doubtless appear that the plant under consideration should produce even more, with basic linings, because it has 10-ton converters, and means of keeping one of them in constant repair, so that the converting operations may follow one another without interruption.

Recapitulation.—1. The endurance of basic linings is so small that the ordinary system of repairs would reduce the output of an American plant about one-half.

2. The only adequate system of repairs, with existing basic refractory materials, is to remove and replace linings bodily by removing and replacing the vessels containing them.

3. Changing converters, trunnions and all requires very costly apparatus, and much labor and time in disconnecting parts and in making the transference.

4. Changing only the shells of converters (leaving the trunnions and their connections undisturbed) requires only cheap and simple apparatus, and the operation may be performed so quickly that basic linings will give the maximum output of acid linings.

5. Leaving the building open in rear of the converters, instead of placing the melting department there, gives good ventilation and ample space for bottoms and shells to be run out for repairs and for slag to be removed from the pit. The cupolas (excepting the spiegel cupolas) may be placed elsewhere, especially by adjacent blast furnaces; and melted metal may be transported thousands of feet without difficulty.

6. Placing the pit bottom on the general level allows slag to be hauled away directly, without rehandling or lifting. The elevation of the converting house floor thus produced facilitates the removal of products.

7. The metal ladles are brought in behind and above the converters, and are discharged by separate spouts, so as to leave the space between the converters open for a short run of the spiegel ladle, and of lime and other solid materials to be charged.

8. The spiegel cupolas are placed near and above the converters, so that the metal may be run in quickly and completely, without vertical or lateral movement, by means of a ladle car.

9. The repairing plant is conveniently placed in rear of the converting house, but it may obviously be modified in extent and position, to suit local circumstances.

THE VALUE OF THE STUDY OF THE MECHANICAL THEORY OF HEAT.

By ALFRED R. WOLFF, M.E.

A Paper read before the American Society of Mechanical Engineers, Nov. 5th, 1880.

In presenting a few remarks on the value of the study of the mechanical theory of heat, I am imbued with the knowledge of two facts: of the importance and value of the study on the one hand, and of, in general, a lack of proper appreciation among engineers of this point on the other. There are doubtless few among educated engineers who would not admit that the acquisition of any form of knowledge is of value, not only as a training of the mind but as an addition to our understanding of the laws and working of the universe and mankind; few who would deny that the study of literature, of social or politico-economic sciences would be of benefit to them, though of limited influence in the practice of their profession.

From personal experience, as well as from the experience of others, I am lead to believe that engineers, as a class, look upon the knowledge of the mechanical theory of heat not much unlike than that of the department of "belles-lettres," as possibly adding to refinement, to a broader view of things in general, to a fair drilling of the mind, but of no practical or only slight practical value in the ordinary, or even extraordinary, exercise of their profession. I am not prepared to say that were the grounds above enumerated the only ones upon which the study could be urged, that a strong argument in favor of its more general introduction could not justly be maintained; but this question does not arise since the more general introduction of the science can be presented to the engineer on the ground of direct, practical utility, for the purposes of correctly appreciating and increasing the efficiency of a large department of his profession, that department which treats of the transformation of the latent or active forces or powers of nature in a form suitable for application as motive power for utilization in machinery designed to do the desired special work, or, in other words, to that department which relates to the generation of the working fluid or motive power, and the work performed by the medium of such fluids in prime movers. It is on this ground that I

would urge the more general acquisition of the science of thermo-dynamics, and to insure conciseness as well as a ready conception of my views, I will only briefly outline the thoughts, giving as it were their direction, and leave it to you to give them force, that is, to develop them more fully as your own understanding and experience will best suggest.

Thermo-dynamics or the mechanical theory of heat is, as the name implies, the science of the laws of heat considered as a form of energy. It is based upon two great, general laws, the one that all forms of energy are mutually convertible in certain exact, invariable equivalents, the other that the quantity of energy in a homogeneous substance is equal to the sum of the energy of its component parts, and that equal parts of a homogeneous substance when undergoing a change of, or exerting, energy undergo like changes, and exert like effects. The application of these two fundamental laws of energetics to heat constitute the determination of the two general laws of thermo-dynamics, which are the basis of the whole science, and of which the science is in fact but an extension and application to forms of heat energy, latent or active, met with in nature and valuable in practice. For after all what is theory but practice reduced to a connected system of laws or principles. Without facts, without phenomena, in short, without practice as a foundation, how could theory ever be conceived or established? In ordinary discussion but little attention is paid to the distinction between theory and hypothesis, and nothing has proved more disastrous to a general appreciation of theory than the repeated misuse of the term. Theory is based on facts, hypothesis on speculation, hypothesis changes to theory when speculation changes to facts. The two special laws of thermo-dynamics, upon which the whole science is elaborated, to which we refer are: that heat is convertible to other forms of energy in the relation of what is known as Joule's equivalent, that is, that one thermal unit (the quantity of heat required to raise one pound of water at 39·4° Fahr., one degree in temperature) is equal to the energy required to raise 772 lbs. 1 foot, or that 772 lbs. falling 1 foot will develop sufficient heat to raise 1 lb. of water at its greatest density 1 degree. The second is that if the total actual heat of a homogeneous and uniformly hot body undergo a change or exert energy, equal parts of the body will undergo equal changes and exert like effects, the sum of the effects of the component parts being equal to the total effect. This law is more popularly identified in the

form of Carnot's theorem, a special application to heat engines, that $q - q_1$ or $\frac{T - T_1}{T}$ is the limit of efficiency of such engines, and that the test of a perfect engine is its reversibility. q and T represent respectively the quantity of heat and absolute temperature of the (any) fluid when leaving the generator, and q_1 and T_1, respectively, the quantity of heat and absolute temperature of the fluid when given off to the refrigerator: $q—q_1$ when entirely converted into useful work causing the engine to become theoretically perfect.

The above outlines the foundation and framework of a great science. It is strong and simple, and in its strength and simplicity beautiful. The structure and superstructure are equally strong, equally beautiful, but not, however, equally simple. If this were the case there would be little need of the presentation of papers of this kind, for the great value of the science would be universally acknowledged. It is a common experience that the mass of mankind primarily pronounce that which is difficult to attain as not worthy of attainment, primarily too often look upon those who have mastered the difficult with scornful or doubting eye, until the achievement of the difficulty by the few has brought forward such valuable results, such striking truths, so influences the ordinary experiences and our conception of facts in nature or in our professions, that the worthiness and value of the attainment can no longer be denied and must be definitely accepted. That point has now undeniably been reached in thermo-dynamics, and the sooner we concede it the better. The sooner we admit that we must study and explore the science of heat energy in its higher form, the sooner will we advance in our profession and contribute to its progress. It will be my aim to recall a few of the practical applications in engineering of the principles which thermo-dynamics has established, and thus to give an illustration or rather indication of its value. But before doing so to be strictly conscientious, I must refer to the difficulties to be met with in the study in its higher form. Professor McCulloch says: "Any one acquainted with only the elements of analytical geometry, and of the fluxional calculus, should find no difficulty in understanding all it contains. In this country, however, scientific education, as well as classical, has unfortunately retrograded; and superficiality is the fashion of the day. Hence, some anxious for scientific knowledge, with the least labor and in the shortest time, imagine it might be well in scientific literature to dispense with the calculus. To them

no better advice can be given than to begin by studying it thoroughly, if they would reasonably hope ever to comprehend much which would otherwise be unintelligible."

These elements, however, must be firmly fixed in the mind, and thermo-dynamics may justly be considered a thorough test of such a knowledge. When beginning the study of the subject, I thought I had fairly mastered calculus, while engaged in its acquisition I found I was a mere novice, and such too has been the experience of a few of my friends who have already progressed and done good work in the science. But let us not be deterred from the acquisition of a valuable subject because of its difficulty. This really incites a person to a hard effort, and the gratification which the mastering of an important point gives, forms an incentive to further advance which acts more potently than the repulsive part of the difficulty. Let us not accept the valuable principles reached without making at least an honest effort to follow the masters who have discovered them, and thus make the principles in reality our own. But if our comprehension and efforts will not permit us to do this, let us not cry down the methods by which they were attained, and let us study the principles themselves thoroughly.

There is a wonderful harmony in nature—without it all scientific research would be of no avail. There are certain great invariable laws, some determined and many more not yet conceived, but in our faith in and knowledge of the reliability of those laws exists the only safeguard of advance. This opens up that large realm of reasoning known as "by analogy," and enables us to determine thus, and by analysis, laws which are not directly determinable from our practical experiences or observed phenomena. Heat, the observed and usually acknowledged primary physical source of all energy (though gravitation might on some good grounds be selected as our present ultimatum) beautifully illustrates this harmony when considered as energy, and thus prescribes laws which are afterwards verified in practice, and to which practice conforms and becomes the embodiment of. This is the strongest plea which it seems to me can be urged for the careful study of the mechanical theory of heat, and we will attempt to point out some of the work which has been done in this respect, and leave it to your judgment to decide whether a science which has already accomplished so much is not capable, when many able minds are devoted to its study, of an application and extension which is and will be of

incomprehensible value in the ordinary practice of the engineer, and which will greatly contribute to the advance of mechanical engineering.

The specific heat at a constant pressure of any permanent gas can be ascertained directly by experiment, not so the specific heat at constant volume. Regnault has made accurate determinations of the specific heat of such gases at constant pressure, and discovered certain laws relative to the same, that the specific heat of such a gas is independent of its temperature and density, and the product of the density and the specific heat is the same constant for all permanent gases. From the velocity of sound, independent of the principles of thermodynamics, the specific heat at constant volume for a permanent gas can be determined, and these two quantities when substituted in an equation of the theoretical value of the mechanical equivalent of heat, expressed as a function of the pressure, volume, dilatability and the two specific heats of a permanent gas give for this equivalent a theoretical value which agrees with that practically determined by Joule, and recently verified by Professor Rowland. I need not dwell upon the mathematical exposition of this, but reference to Stewart ("Elementary Treatise on Heat," pages 330 and 412), Maxwell ("Theory of Heat," pages 169, 228, and 310), Cotterill ("The Steam Engine considered as a Heat Machine," page 82), McCulloch ("Mechanical Theory of Heat," page 92), and Rankine ("Steam Engine," page 321) will serve as a verification of the above summation. Permit me to call your attention to this remarkable agreement between theory and practice. The specific heat at constant volume is not directly determinable from experiment, so it is developed from the velocity of sound, and substituting these two independent experimental data (the two specific heats) in a theoretical equation not involving a previous determination of the mechanical equivalent of heat, we ascertain such equivalent from correct theoretical considerations, and it accords with the experimental determination. What a beautiful correlation of the different laws and facts of nature do we here perceive exemplified.

But this will appear rather as an illustration of the beauty and symmetry of the science than as a presentation of a fact of direct utility to the engineer in the practice of his profession, and since it is my special object to call attention to a few of the latter class of facts, I will have to omit the mention of the interesting relations between the physical properties of bodies, such as between the two elasticities and the two specific heats, gaseous viscosity and the molecular theory of the

constitution of bodies, all of which the science teaches and will at once refer to its main application for our own purposes, the work of fluids in engines. It is in this department of our profession that thermodynamics has made the deepest impression in practice, and is destined to continue it to a far greater extent. It has taught us the different laws of expansion and work of fluids, and has supplied us with the proper test of the efficiency of different forms of engines. $\frac{T - T_1}{T}$, the limit of efficiency has enabled us to correctly appreciate the progress made, and to definitely point out the direction in which the advance in efficiency and perfection of heat engines lies. To increase the theoretical efficiency, T, the temperature of the fluid at its entrance to the cylinder should be raised, and T_1, the temperature at exhaust lowered as much as possible.

The application of this principle to steam engines implies high steam pressures and great expansion. This was tried within certain limits, and the beneficial results looked for were not realized to the extent expected. The theory was still recognized as true by those who grasped it, but the practical result had not conformed to their expectations. The nature, laws and action of steam were thoroughly investigated, and it became apparent that condensation ensued, owing to heat consumed for internal work during expansion and to the detrimental action of the metal of the cylinder. When the steam enters the cylinder (of a lower temperature than the steam at its initial pressure) it gives out heat to the metal of the cylinder, and to do this sets free latent heat, causing water of condensation to be formed. When the steam in the cylinder expands it performs internal as well as external work, and becomes partially liquefied; as the steam leaves the cylinder, rushing into the condenser, the water mixed with the steam evaporates, abstracting additional heat from the metal of the cylinder. When a fresh volume of steam of initial pressure now enters the cylinder it comes in contact with the metal of lower temperature, water of condensation is formed and the action continues as indicated above.

Several methods of decreasing this loss presented themselves:

1. The introduction of the compound engine which dividing up the range of temperature between admission and final exhaust, in two or more stages, causes less difference in each cylinder, and therefore decreases the loss by condensation while again the heat abstracted from the first cylinder at exhaust becomes available in the second cylinder.

2. The use of the steam jacket which tends to keep the metal of the cylinder at uniform temperature and thus prevent the initial condensation of steam, and to supply heat to the steam in the cylinder to prevent condensation during expansion.

3. The introduction of a great number of strokes per minute so that less time will be accorded to the steam to impart heat to the metal of the cylinder and to abstract heat during exhaust.

4. The use of superheated steam, since it can emit heat when being admitted to a cylinder of lower temperature without causing water of condensation to form at entrance or during expansion.

Neither of the first three remedies proved entirely efficacious in practice while all have greatly reduced the loss from condensation. An intelligent study of the theory of heat will show, however, that the latter is the only one which offers the possibility of entirely preventing condensation. Perhaps it will need some explanation why the steam jacket can never entirely prevent this loss. It is owing to the fact that the transfer of heat from the steam in the jacket is not as rapid as the transfer of the heat from the internal sides of the cylinder to the steam in cylinder, that the steam on entering the cylinder heats up but a small thickness of metal to its own temperature, owing to the comparatively poor power of conduction of iron. And the steam in the jacket, similarly, heats up to its own temperature but a small thickness of the metal of the cylinder immediately in contact with it; in brief, the poor heat-conducting power of iron does not allow the transfer of heat from the steam in jacket to the steam in cylinder to be practically instantaneous.

There may be some who are opposed to the introduction of the high speed (as they are popularly termed) or rather "high revolution" engines. But whatever be their practical defects—and they possess some mechanical advantages that we are inclined to think more than counter-balance those defects—the principle is in the right direction, and already splendid workmanship and refinement of mechanism and machinery tends to confirm the value theory accords the system.

We will now briefly refer to the use of air. Thermo-dynamics demonstrates its superiority to steam. Its permanently gaseous nature enables it to expand without doing internal work, and its temperature can be raised to a high degree without the objection of a high pressure difficult to control. But practically the few air engines built have as yet not proved a decided success, owing to the difficulty of obtaining

a rapid conduction of heat to and from the air employed, and the necessity of a larger cylinder than is needed for a steam engine of the same power. Again, very high temperatures of the air cause no inconsiderable strains due to irregular expansion of the metal and a slow oxidation of the metal as well. And so with other forms of fluid engines. Practical difficulties and obstacles have not always permitted the rigid teachings of thermo-dynamics to be precisely realized, though they have in most cases served as an exemplification of its truth. But as long as we have the laws of the mechanical theory of heat to teach us where the possibility of or road to progress lies, better steam and better air and better gas and better fluid engines will in time be built. Even with the fluids and materials at present known, the performances of engines are capable of being doubled, a result by no means too trival to make an honest effort to master the principles which will aid us in securing this end. But who would dare to say in an age when Professor Crookes' fourth state of matter opens up realms of investigation undreamt of, and when Professor Bell's discovery of sound transmitting rays of light potently reminds us that we are but at the threshold of our understanding of the laws of the universe, who would dare to say that new fluids, materials and conditions will not be discovered that will enable us to extend the limits of temperature between which the fluid can expand in a cylinder, and thus utilize by far the greater portion of the latent power which nature has presented to us in her stores of fuel!

But it may be said: The advances, pointed out above, in the efficiency of engines have not in all cases been instituted by those who have mastered the mechanical theory of heat, and the advances, therefore, are not in all cases the result of the study of the subject. This is partially true, that is while, as far as I can learn, no advances have been secured in the efficiency of heat engines (not including, of course, reduction of friction and better mechanism for transforming the rectilinear motion of the piston) by men who have not been intimately acquainted with, at least, the fundamental principles of heat energy, they have not in all cases mastered the whole subject in its highest mathematical form. But to correctly appreciate and define the advance, the theory of heat energy has always been called into play, and if our progress in the efficiency of heat engines has not been as rapid as we might have desired, and is not as rapid at the present time as desirable, it is owing to a lack of knowledge and understanding of the subject

under discussion by some of our more brilliant and experienced minds. There are two methods of gaining knowledge. One by acquiring the laws and results of the experience of others, the other by acquiring the laws by our own experience. Both constitute the acquisition of principles or theory. Both have their uses. But it is a loss of time and it is at the expense of many failures and disappointments which do not contribute to real advance, if we arrive at the same principles by such failures that others have reached before us, by whose experience we might have profited. When we have acquired the knowledge of the work that others have done, we are prepared to make further progress, and if the difficulties and obstacles multiply we will be fairly equipped and fully encouraged to meet them. If we then experience failures, they will contribute to real advance instead of demonstrating a fact or law which had already been acknowledged, and which it was within our power and province to know. Thus too will indorsement of, and investment of capital in, prime movers, advertised as realizing fabulous power be avoided, since our knowledge of the laws of thermo-dynamics will have acquainted us with the principles of the conservation of energy, the impossibility of transgressing certain limits, and will, therefore indicate or demonstrate the fallacies of the projected scheme.

In conclusion, I must say that I am aware that full justice has not been done to the theme under discussion, nor can, in my opinion, full justice be done to so grand a theme within the limits of a paper of this kind. A complete treatise would have to be written to demonstrate its true importance, and some work like that of Rankine is the best verification of the actual value of the science, and in its study does this value become most potently apparent.

But if I have been able to convince—say, one of you, heretofore uninterested, of the *vitality* of the study, I will feel amply compensated, and will offer no apology for my enthusiasm in the cause.

American Watches.—The Waltham Watch Co. has received an order from the British government for 372 watches, to be distributed among the railroad officials in India. This being the third order to the same company is a satisfactory evidence of the esteem in which the watches are held abroad. In each instance the British government advertised for proposals, and the American firm distanced all competitors.—*Fortschr. der Zeit.* C.

BLASTING.

By Arthur Kirk.

Paper read before the Engineers' Society of Western Pennsylvania, Oct. 19th. 1880.

In this age of improvement I know of no important industry in which so much hard labor and money is lost for want of intelligent study and use of modern improvements as in the simple operation of blasting.

When you bear in mind the fact that blasting of minerals lies at the very foundation of all that distinguishes civilized from barbarous life, you will at once admit that its importance makes it well worthy of our attention.

You are all aware that the houses of civilized men are very much superior to those of the barbarous races. The first point of difference is in the foundation, and almost the first things needed for the foundation of a house for civilized man are powder and blasting to get stone for the foundation.

Another point of superiority of civilized life is the abundance of manufactured iron and glass, yet I think that not one pound of all the immense quantities of these articles manufactured around Pittsburgh is obtained without drilling and blasting to prepare either the coal, iron ore, limestone or sand used in its manufacture. But the very nature of the work of quarrying seems to draw to it many men of one idea who work hard and think little; who having made holes in rocks for many years, insist, therefore, that they know all about blasting and persistently oppose all modern improvements.

Many farmers are living yet who have cut many harvests with a sickle before the days of cradles, mowing and reaping machines, etc. These might with the same propriety resist the use of mowing and reaping machines because they know all about farming.

Another reason why improvements are not introduced in blasting is because the owners of mines seldom give any personal attention to blasting, but leave it all to others, who seldom take any interest in improvements, and are often prejudiced against them and condemn them without a trial.

The subject of blasting may be divided into three divisions:

1st. The hole and how to make it.

2d. The explosive used.

3d. The manner of firing the charge.

1st. *The Hole and How to Make It.*—The common way of making the hole is by either churn or jumper hand-drills. Three men usually form a gang, and the gang make from eight to twelve lineal feet of hole per day in hard rock.

By either of these modes it is practically impossible to make a round hole—generally a three-cornered hole being made, which is very objectionable—because, if the hole is perfectly round it will present a uniform circular surface to the pressure, and retain the blast until its force is fully developed. But with a three-cornered hole the pressure on the long sides of the hole is concentrated at the corners, which, being required to resist more than their share of the pressure, give way and permit the explosive gases to escape before their force is fully developed, and only a large amount of smoke is produced.

A much better plan is to have the holes made by machinery; this secures a cheaper and better hole, being perfectly round, and thus retains the explosive gases until their power is perfectly developed, and do more execution on the rock and with less smoke. In close or underground work, where the air is bad, this is found to be of great advantage. With the latest improved steam drills a gang of three men often make from seventy-five to one hundred feet of hole in one day.

2d. *The Explosive Used.*—Explosives may be divided into chemical and mechanical combinations.

Chemical explosives embrace what are known as nitro-glycerine, gun cotton, dynamite, dualin, hercules, Ditmore powder, rend rock, mica powder, etc.

These are produced mainly by the combination of nitric acid, sulphuric acid, and fatty matter or glycerine, with cotton, tan bark, paper pulp, sawdust, soda, mica scales, pulverized charcoal, etc., as absorbents.

These will explode under water, which makes an excellent tamping. Most of them are unfit for sporting or military use, because their explosion is so instantaneous it ruptures the gun before the ball can start. Their force is more like the blow of a large sledge, whose force is spent in an instant, while the explosion of black powder is more like the action of a powerful spring, which starts the projectile more

slowly and continues to propel it until it escapes at the muzzle of the gun.

The manufacture of chemical explosives is of comparatively recent date, and in many cases is as yet imperfectly understood. Many alarming accidents have occurred in the handling of some of them, which has given a bad name to the whole family. But enough is now known about some of them to prove them to be perfectly safe when properly understood and handled, and very profitable in certain kinds of blasting.

Mechanically combined explosives are common black powder, carbo-azoteen and mahoning powder.

These are composed mainly of saltpetre or nitrate of soda, sulphur and charcoal, or tan bark.

All of these ingredients are first finely pulverized separately, and then mixed in certain proportions, and incorporated by being run under heavy wheels or rollers for from two to six hours, so as to mix uniformly and bring a certain proportion of each ingredient in immediate contact with the other ingredients.

The mass is then generally submitted to strong hydraulic or screw pressure, and then is called pressed cake. Then it is crushed by corrugated rollers, and by means of sieves separated into the different sized grains of blasting, sporting or military powder, each of which has its peculiar mark or name.

Carbo-azoteen is made by boiling these ingredients together, and has never been a success.

Mechanically combined explosives must be kept perfectly dry; even a slight degree of dampness injures them.

As a rule, the finer the grain the quicker the explosion and the smaller the charge; while for large blasts in rock, powder is now used to good advantage in grains as large as grains of corn, and in large artillery charges still larger grains are used, called Pebble Powder. We now come to the

3d. *Mode of Firing the Charge.*—Three modes are used—squib, fuse and electricity. Squibs are made of about six inches of rye straw, filled with fine powder, and are used principally in coal blasting. The hole being drilled horizontally three to five feet into the coal a charge of eight to twelve inches of FF blasting powder is generally put in, then an iron rod $\frac{5}{16}$ inch in diameter, called a needle, is

inserted in the hole back to the powder, then the hole is tamped full by having dry clay or sand pounded solid into it, and the needle is withdrawn, leaving an open passage like a pipe through the tamping to the powder. The squib is inserted in the mouth of this hole, and a slow match applied to the outside end; the recoil of the powder burning in the straw throws the burning squib back along the needle hole or pipe to the powder chamber, and the explosion takes place. This mode of firing can only be used in horizontal holes.

Fuse consists of a thread of black powder incased in a wrapping of tarred hemp, and called hemp fuse—or in a wrapping of cotton, and called cotton fuse; then, to make it waterproof, a tarred tape is wrapped spirally around it, and then it is called single tape fuse, and will resist dampness a long time; or a double wrapping of tarred tape is wrapped around it, and called double tape fuse, which will burn and fire a charge of powder under 20 feet of water.

In all of these fuses there is a central cotton thread, which has been saturated with saltpetre, which makes it continue to burn when it would otherwise go out. Every precaution is taken to make the fuse continue to burn until the fire reaches the powder and the explosion is produced. But, from some cause or another, holes sometimes hang fire, and are a source of great danger to the miner or quarryman. Holes thus charged, which ordinarily should explode in from one to three minutes, have been known to hang fire for twenty-four hours and then explode.

The third mode of firing is by electricity. When using it, the hole, the charge and the tamping are the same as in firing with a fuse, but, instead of inserting the common fuse already described, an electric exploder is used, which is made thus:

A copper cap, $\frac{1}{4}$ inch diameter and about 1 inch long, is filled $\frac{1}{3}$ full of fulminate of mercury, into which is inserted the ends of two insulated copper wires connected by a small thread of platinum wire, and the end of the cap is then closed by means of melted sulphur, making it waterproof. Electric fuses are made 4, 6, 10 and 15 feet long, with about two inches of the outer end stripped of the insulation and tinned.

In using them the blaster selects an electric fuse long enough to reach the bottom of the hole and leave a few inches of wire out of the hole, then puts in enough powder to cover the bottom of the hole $\frac{1}{2}$

inch deep, then his exploder, then one-fourth less powder than if fired with a fuse, and tamps the hole the same as with ordinary fuse; any number of holes may be thus prepared of equal or unequal depth or distances apart.

Directions for Firing.—One wire of the first hole is now connected to one wire of the second hole, and the remaining wire of the second to one wire of the third hole, and so on until all are connected; there will then be one wire of the last hole and one wire of the first hole left unconnected. These wires are then connected by means of long conducting wires to the battery at a place of safety, and when everything is ready the circuit is closed and the small thread of platinum wire in the bottom of each hole becomes red hot and each cap is exploded with great force at the same instant, and each hole assists its neighbor. And thus by simultaneous explosion, with only three-quarters of the powder used in ordinary fuse blasting, more than double the rock is moved with the same drilling.

In quarries where fuse is used it is not uncommon to find six inches or a foot, or even two feet of the bottom of the holes remaining unruptured after the blast has been fired and cleared away—the drilling of the unexploded piece of hole was so much hard labor lost, and the powder it contained was lost, for if the powder had done what was intended no part of the hole would have been left unexploded. No part of the hole is thus left unexploded where electricity is used, because in firing by electricity the first point ruptured is at the bottom of the hole, and the full force of all the explosive is spent in enlarging the rupture.

For profitable blasting by either mode of firing, the bottom of the hole in open work should be at least one-tenth less from the open front of the rock than the depth of the hole; thus, if the hole is ten feet deep, the bottom of the hole should not be more than nine feet from the face of the rock, and in some rock not more than eight feet, so as to make the bottom of the hole the weakest part of the blast. But if the hole is ten feet deep and filled up four feet with powder, and fired with a fuse at the top of the powder, there will only be six feet of rock on the top of the first point of rupture, and there being nine feet in front of the hole makes the top the weakest part, and the result is a great discharge of projectiles through the air, to the great danger of all around, and much of the material is lost; but if the

same single hole had been fired by electricity at the bottom of the hole, the bottom rock would have been forced out, and the top rocks would tumble down and do no damage, and all be in place to be handled.

The contrast between firing with a squib and by electricity is still greater, because a fuse may sometimes burn to the bottom of the hole before firing the blast, but a squib must always fire at the top of the powder, and as the needle hole is a full quarter inch in diameter, the first blast is a jet of orange flame from the needle hole, which is partly consumed powder, and cools into dense smoke, to the great annoyance of the miner; in light work, only the top is blown off the hole, the first point of rupture taking place at the top of the powder.

In conclusion, to show this is not idle theory, but actual fact, I may point to the shaft sunk this summer for the Chicago and Connellsville Coke Company, near Uniontown, under the superintendence of Mr. James Harrison.

Mr. Harrison has had great experience in everything connected with modern mining improvements, and on commencing work on the shaft procured the latest and best improved steam rock drill, an electro-magneto battery and safe chemical explosives, and with the assistance of Mr. Thomas, an experienced Welsh shaft sinker, he broke ground about the 15th of last March, and two weeks ago he had sunk the shaft 325 feet to the coal; has got his ovens and all his buildings up, and I presume is now making coke, which under the old plan of sinking he could not have done before next April.

I can also point to the shaft now being sunk by Mr. J. K. Taggart for E. K. Hyndman, near Connellsville, and to another near the same place, now being sunk by Mr. Hopkins for Mr. Wickham, of Connellsville, both of whom are using electricity and chemical explosives to good advantage.

I may further add that theoretically blasting by electricity has been known to engineers for thirty or forty years, for large blasts such as Hellgate. But it has only been within the last few years that the apparatus has been made so simple and portable as to be of any practical benefit in quarry work. It has now been made so simple, and is so easily handled, that a youth can take the apparatus out of the tool box, lay down the cable to a safe place, charge five or six holes, tamp and fire them, and take up the cable and replace apparatus in tool box, all inside of ten minutes.

ON THE WHOLESOMENESS OF DRINKING WATER.

By Reuben Haines.

Abstract of a Lecture delivered before the Franklin Institute, December 9th, 1880.

It was only about thirty years ago that cholera epidemics were discovered to be largely due to the transmission of the disease from one person to another by means of water used for drinking. This disease is unquestionably transmitted also in other ways, as for example, by the atmosphere; but that water is one of them, and in some countries the most important one, the vital statistics of England prove beyond a doubt. I say, in some countries, not in all; for it appears that the evidence in Germany, which seems to have been very thoroughly investigated, is against the theory of the carriage by water of the cholera poison in such a way as to cause infection. Pettenkofer, the celebrated sanitary authority of Germany, is entirely opposed to this view. Nevertheless, in England, India, Holland, and a few localities in Germany, the evidence appears overwhelming. Dr. Parkes states that while we should give proper deference to the evidence in Germany and Austria, we should not allow it to outweigh the evidence from other countries.

It has also been found that typhoid fever has been spread through towns and smaller communities and through separate households by carriage of the infection by drinking-water. So many instances of this have become known that it may be considered proved beyond all possible doubt to be a fact, and one which is generally and not merely exceptionally true.

It is true, however, that typhoid fever, like cholera, may be transmitted also by the air; and there can be no question that the gases or vapors emanating from sewers and drains and breathed in a confined atmosphere are a very frequent means of transmitting the disease.

Dr. Parkes states, in the latest edition of his work, that the question which is the most important or frequent means of infection, air or water, cannot yet be answered. Dysentery has been long known to be caused partly by bad water. There is considerable evidence to show that water may transmit diphtheria, but not sufficient to prove it with

certainty, except perphaps some evidence in Massachusetts which is very strong.

When the water in any of these cases has been examined by chemical tests it has generally been found to have organic matter either dissolved up in it or suspended in it. Frequently this material has been found so entirely dissolved as not to be detected by the eye; that is to say, water which was clear, colorless, free from anything visible, except a very few floating particles and which even had a good, refreshing taste, was found on careful examination of all the circumstances to be without the slightest doubt the real cause of the spread of typhoid fever and cholera from one person to another.

Water, on the other hand, which is pure or free from any contamination with human sewage, in any part of its history in past or present time, has never been proved to be the means of infection with such diseases as cholera and typhoid fever. But diseases of other sorts have been caused by mineral matters dissolved in the water or by vegetable matter held in suspension, while the water was nevertheless entirely free from sewage contamination.

Water, then, which is impure, is to be dreaded as a frequent cause of disease. But in what does this impurity consist and how are we to distinguish the two sorts of impurity I have mentioned? We will understand this better if we first carefully consider what constitutes a naturally pure water and which is found, by wide experience, to be perfectly wholesome and should be our daily drink.

Those who have studied chemistry are aware that absolutely pure water is composed of one part by weight of hydrogen to eight parts by weight of oxygen, or, by volume, of two parts of the former to one part of the latter. Chemically pure water contains nothing else whatever. But such water does not exist in nature, nor can it probably be produced in the chemical laboratory, for the purest distilled water, redistilled many times, is found, perhaps invariably, to contain exceedingly minute traces of ammonia and on standing a few hours it absorbs oxygen and nitrogen from the air to which it is exposed.

When, therefore, we speak of a pure, wholesome water we do not mean water which is chemically pure, but one which is as free from foreign substances as is to be found under the most favorable natural conditions.

We should consider all natural water found either on or below the surface of the ground as having been originally precipitated out of the

atmosphere in the form of rain, snow, hail, fog or dew. Of these, rain and snow are obviously the most important, and these in falling carry down with them a part of whatever may be either naturally or abnormally present in the atmosphere. Now, we know by practical experience that the atmosphere in high situations is generally purer than on level plains or in low places. When our bodies are in need of an invigorating atmosphere we go to the mountains, and those who have traveled in mountainous regions often speak of the delightful effects experienced, provided they do not enter too rare an atmosphere. Chemical analysis confirms this impression. The atmosphere of the mountain is generally really purer than that of the valley at its foot.

Rain collected near the surface of the earth will, therefore, be more impure than that collected at a considerable height, because of the greater impurity of the air near the surface of the ground. After the elapse of a short time the rain water will be much purer than during the first part of the time of rainfall.

The purest air of the mountain regions contains, besides nitrogen and oxygen, a small proportion of carbonic acid in the free state and still smaller amounts of ammonia combined with carbonic acid, nitric acid and nitrous acid. Besides these, there is found also a very small amount of organic matter suspended in the air, probably dead or effete matter swept up by currents from living animals and from vegetation in decay. The amount of this organic dust becomes less and less as we ascend to greater heights, while the proportion of carbonic acid becomes somewhat increased. All of these various gaseous and solid substances are therefore to be found in rain water. Since a part of the rain goes to form springs, we find these substances in the purest spring-waters; but here they exist in a somewhat different form from that which they had in rain. Water in passing through the ground always comes in contact with some substances capable of dissolving more or less in it.

Water in its purest state may be said to be an almost universal solvent. Give it sufficient time and the proper temperature and atmospheric pressure and it will dissolve an appreciable amount of the most insoluble substances. This effect will take place much more readily when the water contains certain saline substances, especially nitrates and chlorides. When rain water has carried down from the atmosphere salts of ammonia, even if in very minute amounts, its solvent power on soil and rock is thereby increased, and this increase will be

in some proportion to the amount of impurity washed out of the atmosphere.

The soil itself is known to contain carbonic acid in its pores in much larger proportion to the other gases than exists in the atmosphere. Carbonic acid, we all know, is soluble in water in greater amount, according to the pressure, as is well shown in the soda-water fountain. This gas when dissolved in water has a great solvent power on limestone rock, and when the carbonic acid gas escapes again by evaporation the water leaves those beautiful pendant stony icicles which we find in limestone caves like the Mammoth Cave of Kentucky and which we see on a very small scale on the arched roof of many stone bridges, where they are formed from the mortar between the stones of the arch. In fact, the caves themselves are thought to be formed by this solvent action. Lime is a part of the material of many other kinds of rocks beside limestone and of the soil resulting from their disintegration. The gaseous carbonic acid of the soil added to that already in the rain and to the nitrous and nitric acids in combination with ammonia also found in it, dissolve a part of the lime and other mineral matter of the soil and rock, and when this soluble mineral matter is in large amount a mineral spring is formed thereby or simply an ordinary limestone spring, as the case may be. In either case the water is called "hard." If the rock is of insoluble material like granite or gneiss, only a very small part of it is dissolved, and in this case a soft spring water is formed. Yet even in granitic regions the water may be hard, owing to such substances as sulphate of lime in the soil above the rock being dissolved in it. This is to a certain extent the case in Germantown, where some of the uncontaminated wells furnish quite a hard water while others give very soft water.

All soils contain more or less organic matter derived from vegetable and animal matter in decay. Some of this will necessarily go into solution and pass into the spring water; but it appears that some spring waters contain less organic matter than the rain from which they are derived, so that there is possibly a filtering action going on in the soil in these cases. In the majority of cases, however, it is probable and in many cases, it is certain, that a proportion of the organic material of the soil is added to that already in the rain. The amount of organic matter in soil and rock varies according to its geological nature. Thus, such rocks as the granitic series contain almost no organic matter, while sands and gravels, sometimes thought very free from it,

generally contain considerable amounts of it, and occasionally in so large amounts as to cause the well waters to be decidedly injurious. Dr. Parkes mentions as an instance of this the district in the South of France called the "Landes," where the sandy soil contains so much as to cause water of that region to produce malarial fever.

Alluvial soil, or that deposited by the floods of rivers and streams, contains very frequently large amounts of organic matter which passes into springs and wells and renders them unwholesome.

Waters from marshes are well known to contain large amounts of vegetable organic matter, so as often to prove extremely unwholesome. The case of the ship *Argo*, sailing in 1834 from Algiers to Marseilles, may be mentioned as a very strong instance in proof of this.

Water coming from peat bogs is often highly colored with dissolved peaty matter. This peaty matter appears to be very different from the vegetable matter found in marsh water in not being in a state of actual decomposition, but being material which has in the solid state become partially carbonized and undergone thereby a change preparatory to becoming coal. Hence a number of chemists and sanitary officers have of late years strongly protested against the condemnation of peaty waters on the same grounds as marsh waters, declaring this to be a great mistake. It must be admitted by every candid student of this subject that some waters, which experience has proved to be very wholesome, originate in peat bogs and contain in the state used for drinking very considerable amounts of nitrogenous organic matter in solution, and there is evidence to show that this material is mainly of vegetable origin.

Let us now consider the effects produced on the health by the mineral matter commonly present in ordinary spring and river waters under entirely natural conditions. While we can positively say that our present knowledge does not warrant the assertion that any one of a number of these mineral substances have any injurious influence upon the health, it is nevertheless obviously true that a large excess of lime and magnesium salts, such as carbonates, sulphates and chlorides, are undoubtedly unwholesome in many instances.

Waters which contain these salts of lime and magnesia are called "hard" in consequence of their action on soap. The great waste of soap caused by these waters as well as their injurious influence in the boiling of meats and vegetables are matters of considerable importance in domestic economy. The soap waste and the tendency to

form incrustations in boilers are facts of vast importance to the manufacturer, but these considerations we will postpone for the present.

While dyspepsia and other internal maladies have undoubtedly been traced to the drinking of water containing large amounts of these salts and while goitre and cretinism occur in close connection with, and some affirm, are caused by magnesian limestone waters, there seems to be still a decided disagreement among sanitary authorities as to the wholesomeness of a moderately hard water. Hardness, however, which is due chiefly to sulphate of magnesia, would seem certainly undesirable on several accounts.

From these circumstances it will appear evident that the purest water naturally contains some appreciable amounts of mineral matter, and a water which does not contain it in too large amount, the limit of which has not been satisfactorily settled, may correctly be considered, so far as this is concerned, a perfectly pure water from a strictly sanitary point of view.

Finally, from this whole discussion it is clearly seen that a hygienically pure water must be defined as one which may contain naturally foreign substances, both mineral and organic, in small amounts; but if the organic matter is in larger amounts it must not be in a state of decay or decomposition or capable of readily undergoing such changes.

Further consideration will lead us to modify this statement in so far as to add, that a pure water must also contain nothing which would leave a suspicion upon the mind of a probable present or future contamination with sewage.

Some authorities would add that it also must contain no evidence of contamination with sewage *in any past time.* This is the position which Dr. Edward Frankland takes; but I think we may reasonably dissent from so severe a decision, which would exclude all our river waters as too dangerous for public supply. The chief reason for dissenting is that the premises upon which he founds his conclusions have not been clearly proven, indeed, they are directly disputed by other chemists.

We have heretofore considered water as it exists under wholly natural conditions. We will now consider it as these conditions are influenced by man's occupations and habits which are consequences or accompaniments of civilized life, and interpose circumstances more or less artificial.

We have noticed the influence which the natural atmosphere has on

the chemical condition of rain water. What is now the effect of the atmosphere surrounding communities of men?

The atmosphere of cities and towns is rendered impure by the gases, smoke and soot issuing from the chimneys of houses and factories; by the dust from the streets, which is to a great extent organic; by particles of all sorts of clothing, hair, skin and refuse material from every trade; organic matter from the breath and persons of men and animals; and also by the gases resulting from decomposition of organic matter such as garbage and filth usually left for some time in places where it should not be at all, and the gases arising from stagnant sewers.

Rain, in falling through such an atmosphere, will wash down these gases and organic dust, thereby rendering the atmosphere more fit to dwell in. But the rain water itself will be exceedingly impure, and, indeed, so foul as to be entirely undrinkable without nausea. Beside the organic matter, nitric, sulphuric and muriatic acids, in the free state as well as combined, are found in such rain in considerable amount, which will vary greatly. In addition to the effete or dead organic matter we find also in the air minute forms of animal and vegetable life. Nevertheless, the air of well ventilated streets, even in large cities, is much purer than we might expect. The percentage of oxygen in many streets in the less crowded parts of London, that is to say, outside of the limits of old London, was found very slightly less, and the percentage of carbonic acid very slightly greater, than the natural amount on open land.

Rain falling in suburban towns will, of course, be much purer than that of cities; but where the houses are situated near much-traveled streets, dust composed largely of organic matter will be deposited on the roof, and the rain water collected from this will contain considerable impurity.

If the gutters and pipes placed to carry the water into cisterns, or the cisterns themselves, are made of lead or zinc, these metals will be dissolved by the water so as to render the cistern water poisonous to drink. This corroding action is commonly much more rapid with rain water than with ordinary spring water.

If we pass on to the consideration of other kinds of drinking water, we find that streams, ponds and rivers, all of which are used for this purpose, contain the dissolved manure from cultivated fields, and the filthy and often very poisonous drainage from factories of

various kinds, besides those which make use of chemicals in their processes of manufacture. In addition to these, a most important source of impurity, and wherein lies probably by far the greatest danger, is the sewage of cities and towns situated along the banks of rivers or lakes used for drinking water, or on streams draining into them. Frequent reference is made to the solid impurities, such as wool factory refuse, floating on the surface, but any one who is acquainted with the methods used for cleansing the raw wool knows the disgusting character of the materials used for this purpose, enormous volumes of which are poured into the river after the operation is finished, and which are probably entirely unseen because dissolved in the water. It may be said, however, that much of this organic stuff may be entirely decomposed and destroyed before it passes through the water mains and is delivered to consumers. We hear frequent remarks made on the impropriety and danger of allowing cemeteries of large extent, like Laurel Hill, to drain into a river water used for drinking. The dangers arising from this source may, however, be said to be infinitesimal as compared with the direct drainage of human bowel excreta into the river by means of city sewers. The peculiar dangers of sewage contamination of public water supplies will be more clearly understood as we discuss the subject in subsequent lectures of this course.

Wells are the chief source of drinking water in country and suburban districts, and in towns having no public water supply. Artesian wells of great depth are sometimes used for city supply, but these are found unsatisfactory, because they do not meet the demands of an increasing population, and at the same time generally furnish a very hard water.

Deep wells, or those over 100 feet in depth, but not artesian, are seldom used for private domestic purposes, yet these are strongly advocated by high sanitary authorities, as furnishing the best supply for domestic use where a "driven" tube well cannot be sunk. The chief object is to go below and to exclude carefully the water in the upper water-bearing strata, which will be very liable to become impure. The wells generally used are less than fifty feet deep, and very frequently both these wells and the vaults and sinks for sewage are generally placed tolerably near the house, and also near each other. Often in small lots, and sometimes in larger ones, the cesspools or privies and wells are not more than ten feet apart. Ash and garbage heaps, in a state of fœtid decomposition, are frequently placed quite close to the well. All

this has been found actually true in various parts of England, and in Massachusetts.

As might be expected, numerous cases of typhoid fever were the immediate cause of the investigations that were made. Not only in Massachusetts is such culpable negligence to be found, but also in many other parts of this country, and as near home as the immediate neighborhood of Philadelphia. The recent investigations of the Massachusetts State Board of Health into the sanitary condition of hotels and boarding-houses at various summer resorts of that State has brought to light a large number of cases of most astonishing ignorance and criminal negligence in this respect on the part of both hotel proprietors and smaller householders. Some of the cases discovered on the island of Martha's Vineyard were so bad that it is difficult indeed to believe that any person would knowingly subject himself to such dangerous surroundings.

Dr. Joseph G. Pinkham, in his very able and thorough official report, in 1876, on the sanitary condition of Lynn, Mass., himself a resident of that town, makes the following remarks:

"The most erroneous ideas in regard to the liability of wells to contamination prevail among the people. Those who are familiar with the principles of under-drainage by means of porous earthen tiles know that when they are placed in the earth the water will find its way, for quite a long distance on either side to them and through their pores; yet they are only small vacant spaces in the earth, while a well is a large and deep one, attracting moisture from a much greater distance. But, notwithstanding these well known facts, persons of high intelligence on most points feel perfectly secure in regard to their wells with a cesspool or privy within a few feet of them."

In regard to other sanitary conditions, Dr. Pinkham remarks:

"Less than one-tenth part of the families, shops, etc., supplied with the city water have drains connecting with the sewers."

He estimates that drainage water to the amount of 420,000,000 gallons are annually absorbed by the soil of this town, and then he asks the reader to form his own opinion as to the probability of this foul drainage soaking into the thousands of wells situated in this same thickly settled part of the town. There is but one possible answer to the question. It is but right to add that what was true of this town in 1876 may be very much improved now, but of this I have seen no positive statement.

We know very well that any hole or ditch acts as a drain to the earth surrounding it. A well, as ordinarily constructed, is precisely such a hole for drainage. Any contaminating liquid, or any solid matter capable of being dissolved and washed into the soil by the rain; any such material as human sewage placed or allowed to flow on the surface of the ground near the well, will be exceedingly liable to pass directly into the well water. In other words, it will help feed that well, and a considerable amount of sewage escaping filtration will eventually be daily consumed by the people of the house in their drinking water.

This is certainly not a pleasant subject to contemplate, but it will do us no possible good to shut our eyes to a state of things which actually exists, and which is every day liable to cause disease and death in our families.

The more loose the soil in which the well is dug, that is to say, the more sandy and gravelly it is, the more liable the well is to contamination. It has been stated by some writers that a well drains a mass of soil in the shape of an inverted cone whose apex is the bottom of the well and whose base is an area of surface having a diameter equal to three times the depth of the well. That is, a well of 20 feet deep will drain an area 60 feet in diameter or any liquid within 30 feet of the well will be liable to pass directly into it. This statement is necessarily a very rough estimate, since the area of surface drained will vary exceedingly according to the character of the soil. There is clear and positive evidence to show that in sandy and gravelly soils the extent of drainage area is far greater, even when the surface of the ground is level and the stratification of soil is quite horizontal. In New England the Massachusetts State Board of Health Reports give a number of instances where pollution of wells from cesspools situated in sandy level soil to the distance of more than 100 feet from the wells. The water was found by chemical analysis to be polluted with sewage and cases of typhoid fever occurred from the use of such water for drinking.

If contamination takes place in level soil to the distance of 100 feet it will undoubtedly be liable to occur at much greater distances when the soil and rock are inclined towards the well. Mr. Child, Officer of Health for Oxfordshire, England, gives in one of his reports an instance of the fouling of wells by petroleum or benzine which passed through the soil from a broken barrel buried in the ground to a num-

ber of wells all of which were from 250 to 300 yards distant. The surface of the ground had a descent of about 60 feet between the two points toward the wells. About 82 people living in 15 houses were unable to use these wells for ten days and cattle refused to drink from one of them. In commenting on this remarkable case we should recollect the extraordinary penetrating power of petroleum oils. We can scarcely believe that sewage can be carried through nearly the sixth of a mile of soil as the petroleum was in this case. Nevertheless it furnishes a very good text for a sermon on the possibilities of sewage contamination.

Sand and gravel at first undoubtedly exert a certain amount of filtering power. But this is soon exhausted by the soil becoming saturated with filth, and in course of time a direct channel may be opened to the well through which the sewage may sometimes pass unobstructed even in the solid condition.

From these facts we can positively say that a sandy and gravelly subsoil is one of the most dangerous of all situations for a shallow well in a thickly settled neighborhood, or where the cesspools and privies are not placed at the distance of several hundred feet from the well.

A "tube" well, or "driven" well, will be little, if any, better whatever, unless the tube is sunk to a depth approaching one hundred feet, except in a few cases.

In addition to this we have already seen that such soils very often contain large amounts of organic matter, naturally present, and that this amount may be of itself so large as to produce malarial fever, by drinking the water. These considerations are of especial importance in regard to the well waters of southern and central New Jersey, and in regard to the possibilities of obtaining a really and permanently pure water supply, public or private, at the seaside resorts of that State, which must be considered by the sanitarian to be decidedly a doubtful matter, until careful chemical investigation can prove the contrary to be true.

It has lately been discovered in Massachusetts that some kinds of rock are no real hindrance to the direct percolation of cesspool sewage through them. In one case the well was sunk down into the solid rock for some distance, and the walls of the well were with the utmost care laid in mortar and coated with hydraulic cement from the surface of the ground down to the rock. Yet with all this care contamination took place from a cesspool on the opposite side of the house and fifty

feet distant from the well. The sewage was shown to have passed down through the rock at right angles to the dip or inclination of the rock, which was about 45°. The rock was sandstone, and percolation took place between the joints or fractures in the strata. On the removal of the cesspool to a distant part of the premises the water became decidedly better. The account of this remarkable case was published by Dr. Pinkham, of Lynn, in *Scribner's Monthly* for 1877.

From the brief study we have made of this subject in this lecture we may sum up the following conclusions to which we should carefully give practical attention.

On no account should we ever allow a cesspool, vault or surface privy within a radius from a well equal to twice its depth.

No drain pipe, whether of iron or terra cotta, should ever be allowed within this distance on account of the danger of leakage. The best laid drains have been frequently found to leak in the most unexpected places, and where especial care had been taken to prevent it.

In the case of houses on small lots of ground in a town wells ought never to be used at all for drinking. The Board of Health of such towns should have full legal power to close up all such wells, and remove the pumps, even if no positive disease has ever arisen from their use, or even if no complaint has been entered by a person residing in the immediate neighborhood. The Board of Health should also have power to prohibit any new wells being dug on premises of less than a certain fixed size.

Where a system of sewerage exists cesspools should be totally prohibited under penalty of law, unless such cesspools are built absolutely water tight, and kept so, and are thoroughly ventilated by tall pipes or chimney stacks, and the contents are frequently and regularly removed.

A certificate of compliance with these conditions given by regular official sanitary inspectors should be required at regular stated periods, say once a year, of every person having such cesspools on his premises.

No new cesspools should be allowed under penalty of law, unless by a certificate granted by a regular official sanitary inspector.

When a system of sewerage does not exist in a town the same law should be made to apply wherever the houses are not very scattered. In rural districts the law would be inapplicable.

All wells located near sewers or under street pavements, or exposed to contamination from street gutters, should be immediately closed and

the pumps removed. Such wells are extremely dangerous and a direct menace to the public health. In cases where a right of way to such a well is granted to several property holders by deed of title the Board of Health should have legal power to overcome resistance to its action so as to prevent all further use of the well, under penalty of law. A case of this kind has lately become known within the limits of Philadelphia, which I was requested to investigate.

In all cases of refusal of property holders to comply with such requisitions of the Board of Health, the latter should have power to proceed as in ordinary cases of public nuisances.

A modification of the law should be made so as to include within its prohibition all such wells as are likely, in the opinion of regularly constituted sanitary authority, to become polluted in the near future as well as those which are now actually in a polluted condition.

[In his concluding remarks the lecturer spoke of the minute living animals found in river water, and exhibited by means of the lantern several photographs of animal and vegetable life found in the water supplied to London in 1851 and 1854, which were years when cholera was violently epidemic in that city.]

These photographs are taken from plates in Dr. Hassal's work on adulteration of food. I wish to express, however, my strong dissent from the opinions of Dr. Hassel as to the injurious character of these animals and diatoms themselves. I believe that no clear and positive evidence whatever has ever been brought forward to prove with any degree of certainty these forms of life themselves to have been the cause of disease. They could not have caused cholera in London, for many of them, nearly all perhaps, are stated to be found in the Schuylkill river water at the present time, and yet Philadelphia was, at least in 1872, exceptionally free from cholera at a time when it was epidemic in other parts of the country. No disease has ever been proved attributable to these living forms in the Schuylkill river.

The chief point to be considered is, what may be associated with these animalculæ and upon which they may feed, or in some way be the indication of its presence. Some of them, undoubtedly, live only in comparatively good water, in proof of which statement, the reader is referred to the work of Dr. Macdonald on the microscopic analysis of water. Some of them require a polluted water, and one which contains sewage in not too great amount will often be crowded with some

forms, as was the case with some of the London water, which in 1851 and for some years after, was taken from the river at the London bridges, where contamination with sewage was very great.

These animalculæ, therefore, may be an indication of organic filth in the water, and this filth may be excreted from the intestines of diseased people. The animalculæ themselves are probably no more injurious to a human being than raw oysters. In all probability they can be digested with equal rapidity, and there is, as far as we know, no possible way for them to get into the blood in a living state.

[Photographs were also exhibited of the fresh-water algæ which were the cause of the disagreeable pig-pen odor in the Boston water a few years ago. These were from plates drawn by Prof. Farlow, of Harvard, for his paper in the Massachusetts State Board of Health Report for 1879.]

Electro-motive Force of Amalgams.—Hockin and Taylor find that potassium, sodium, cadmium, tin and copper become more strongly negative, iron and zinc more strongly positive by amalgamation, whether in acidulated water or in sulphate of zinc. Mercury retains hydrogen more strongly than other metals; its surface can be depolarized by a trace of sulphate of mercury or of bichromate of potash. In this case the electro-motive force of mercury, which contains a little zinc, varies from ·24 to 1·498; a very small quantity of positive metal dissolved in mercury can therefore greatly modify its place in the electric series.—*Les Mondes.* C.

Mutual Action of Magnetic Needles in Liquids.—Obalski has recorded a pretty experiment with two magnetic needles, suspended above a vessel of water by a delicate thread, and removed from one another by a distance slightly greater than the sum of their radii of mutual attraction, their opposite poles being near each other. A caoutchouc tube, filled with water, allows the level to be gradually raised or lowered without the slightest agitation. As soon as the needles are immersed they approach each other, on continuing to raise the level of the liquid the approach increases, and finally, when the submergence has reached a third or a fourth of the needles, the two needles rush together. A similar phenomenon occurs when the needles are suspended by their like poles. While in the air their mutual repulsion is hardly perceptible, but the free extremities gradually move asunder as they become submerged.—*Comptes Rendus.* C.

AN INQUIRY INTO THE LAWS OF THE BEAUTIFUL IN MUSIC.

By H. A. Clarke,
Professor of Music in the University of Pennsylvania.

Paper read before the Utopian Society, with additions, December, 1880.

The laws of the beautiful in music are very difficult of ascertainment, owing to the total absence of any objective standard with which the results of this art may be compared. We judge of the poet by the power with which he presents what human experience recognizes as truth, or by the keenness with which he analyzes passions with which we are all familiar. The sculptor has the form of man and of animals as a standard with which to compare his work. The painter has the endless variety of nature to guide him. Hence these arts are imitative—not, in the strictest sense of the word, creative. But in the art of music none of these helps are to be found. Outside of ourselves music is non-existent. Nature, with all her endless variety of sounds—beautiful as many of them are—has no music, gives us no hint of any standard with which to test the creations of the musician. The songs of birds, the murmur of streams and all the sounds of animate nature owe their beauty chiefly to circumstances and association, and are wanting in the first element of music.

We are then driven to the conclusion that music is a pure creation of man. Nature has taught him nothing. Other arts find their material and their motive ready and waiting for them. The beauty of nature would be the same had poet, sculptor and painter never lived; but the beautiful in music had to be slowly and laboriously evolved by man himself. He must first select from the great mass of sounds those that will form a scale—an operation that took some five thousand years. He must devise laws for the combination of these sounds. He must patiently spend centuries in devising instruments for the production of these sounds, and must learn by ceaseless effort to use these instruments. And even when all this is done the result may not be music, or may be bad music, which is the same thing. This brings us to the question. Why is *this* good music? Why is the other bad music? Are not both straitly conformed to the rules?

Yes. Cannot both be equally well performed? Yes. Then what is the intangible something which makes such a vast gulf between them? We cannot, and never will be, able to say. For want of a better explanation we call it genius; but this only removes the question a step further. As I said before, man has had to make his own standards in this art, and the inevitable consequence of having no independent standard is, that the ideal of good music has varied and always will vary while humanity exists. The most we should say of any composition is that the majority of those *best* qualified have consented to its superiority. In short, no definition of good music can be given until we can trace with unfailing precision the connection between sound and emotion, or sense and sensation.

It is comparatively easy to account for the emotions excited by the sister arts. They appeal to universal experience. As an illustration, the pleasure of looking at a well painted landscape is chiefly made up of memory. We all have memories, more or less vague, of beautiful scenes of wood, or seashore, or mountain; these memories are appealed to by the painters, and our gratification is in proportion to the truthfulness, as we call it, of the appeal. We thus have at hand a means of checking our impressions by this hardly conscious reference to well known standards.

But what experience or analysis or memory will account for the emotion excited by a Beethoven symphony? What is it when reduced to its elements but a heterogeneous mass of sounds, of various pitch, quality and intensity. All attempts at explanation have failed and must fail. One of the greatest of philosophers has tried to prove that the imitation of natural sounds is the origin of music—falling into the mistake so common to scientific men, of confounding *sound*, no matter how agreeable, with music; whereas, sounds bear the same relation to music that pigments do to painting.

But although we are unable to say what constitutes beauty in music, we are able to state with some confidence certain necessary conditions to which it must conform, or it could not be beautiful, still remembering that it may fulfill these conditions and yet resemble music only as an architectural drawing resembles a picture.

The first and the fundamental law of music is rhythm. Rhythm in sound means the same thing as symmetry in form. Symmetry, as defined by Ruskin, is the reproduction at equal spaces of the same figures and the same arrangement. Rhythm is the reproduction of an

accent at equal intervals of time. It is strange that the flight of time should be one of the essential conditions of this the most evanescent of the arts. We are not without proofs that rhythm is the true genesis of music. The music of all savages and of the ancient civilizations was largely composed of pulsatile instruments as the sistrum, crotalus, drum, cymbal and so on. We have here the first quality that distinguishes music from natural sounds; no sound in nature, not even the songs of birds, is rhythmical. Philosophers have tried to explain this inherent love of rhythm and symmetry in the hypothesis that space and time are so vast that the human mind shrinks from any attempt at their comprehension as wholes, and feels the need of some recurring points of rest, without which it is lost in their vastness.

I must digress here for a moment to give my reasons for believing that music originated in the love for rhythm, not in the attempt to imitate natural sounds. Mere noise, when rhythmic, is a great exciter of the passions, particularly of the savage's master passion—the love of fighting. Hence all nations, savage and civilized, have used and still use drums, cymbals, gongs, etc., as a necessary part of their war material. It is a common custom with savages when sitting round their camp fires or at their solemn "pow-wows" to recite the deeds of their warriors or ancestors, the recitation being accompanied by the beating of drums in regular rhythm. This recitation would soon accommodate itself to the drum beat and would develop into a rude chant. The voice would naturally rise when describing how the hero slew his foes and would fall when lamenting his death. The repetition of the same story, with the same inflections of the voice, would give rise to a rude melody. It would seem that the ancient Greek music arose in this way because it was lacking, as far as can be known, in *musical* rhythm, *i. e.*, the rhythm depended on the poetry, the musical sounds having no definite value. We have here the practice of recitation to musical sounds carried to its highest development. They, the Greeks, neglected the rhythmic accompaniment and bestowed all their attention on the reciting chant. It is to them we are indebted for all the forms of chant we now possess, viz., the Gregorian or Ambrosian and Anglican. It is strange that with their keen artistic sense they should have taken the wrong direction in this art and never made the discovery that rhythm in the *music*, not the *poetry*, was the true path to the highest development of the musical art.

But although cultivated music took this direction, the "Folk-music,"

i. e., dances and songs, were all of necessity conformed to rhythm, and it is to them that our modern music must trace its origin. The feeling for the combination of melody with rhythm gradually displaced the chant forms, and in the thirteenth or fourteenth century first claimed the attention of cultivated musicians, and has since gradually, but surely, displaced the older form which is now only retained in civilized countries in a modified form for religious purposes. In short, a close study of the history of music seems to indicate that it began with rhythmic noise produced by drums or even by striking two sticks together—to this was added recitation—this developed into the chant, consequently vocal music, as we know, was first developed. But, the universal practice of dancing also required some rhythmic accompaniment. This the drums, triangles, etc., would also supply, still united with voices, which would be compelled to measure their lines or sentences in such way as not to interfere with the rhythmic movement of the dancers; as instruments improved the use of voices for this purpose would gradually disappear, and from this has grown measured instrumental music; in proof of this all the compositions for instruments, with few exceptions until the close of the eighteenth century, were collections under the name of "Suites" of dances, such as gavottes, gigues, sarabandes, etc. So the savage had rhythm without music, the ancients music without rhythm—it was left for the moderns to combine the two and develop all the resources of the art.

The next law—one degree higher—is melody. This is dependent on the first law, and consists of sounds of different pitch adapted to some predetermined rhythm. Melody is one remove farther from natural sounds. It presupposes a definite arrangement of sounds called a scale, a thing that does not exist in nature, as every one knows who has tried to write down the songs of birds, all of which are constructed of intervals that do not exist in our scales.

The third and a still higher law, but still dependent on the two previous, is harmony, or the art of combining sounds. Melody and harmony are to each other as drawing is to painting. Drawing may exist without painting, but not painting without drawing—or, at least, it is very poor painting. So melody may exist without harmony, but harmony without melody makes very poor music, if it deserves the name at all.

Again, a skilfull drawing will, as painters say, suggest the color, and give an exact representation of the object; but the completed

painting, with its gradations of light and shadow, its blending and contrasting of tints, seems to bring nature herself before us. So a beautiful melody heard alone will give suggestions and hints of its capabilities of expression; but with the composer's harmony it brings before us his whole thought, with every shade of expression enhanced by the tints of the harmony. We are now far beyond nature, which has no hint or suggestion of musical harmony. It is man's own kingdom. He created it after centuries of work, and has possessed it for little more than two centuries. The fourth and highest law in music is form. As we advance, the mystery that surrounds the art deepens instead of clearing up. Who can say by what process of reason or instinct we have arrived at what we recognize as the highest form in music—the sonata? Why have we decided that the theme that is suitable for sonata treatment will not do for rondo treatment, and the reverse? Or, why is the sonata a higher, nobler form than the rondo? Form is an extension or development of rhythm. We have first the rhythm of the bar, then of the phrase, then of the theme, and lastly of the alternation and recurrence of the various themes. It might be compared to the revolution of satellites round their planets, of planets round their suns, and of suns round their unknown, unsearchable centre—*the ideal* of the composer.

I have now stated all that is definitely ascertainable concerning the laws of the being of this art. It seems very little; but when we remember that it is the unaided creation of man, we have a truer appreciation of the difficulties that he has had to overcome since he first attempted by rude drums and cymbals to divide time rhythmically, or by pipes and strings to make weak attempts at melody, or by barbarous successions of fourths and fifths essayed to combine sounds into harmony. From these rude beginnings has grown this most perfect of the arts. And it would seem in our day that its forms and means of expression have been all elaborated to their greatest possible perfection, and that no advance can be made until a *new* scale and a *new* harmony—of which we cannot even conceive or see any indications—be evolved.

Silver Plating.—In plating German silver H. Krupp, of Vienna, first deposits a galvanic coating of nickel, next a coating of copper and finally a coating of silver.—*Dingler's Journal.* C.

Photographs of Nebulæ.—Janssen, in congratulating Draper upon his successful photograph of the nebula in Orion, recommends that the greatest possible number of photographs should be taken, in different observatories where there are suitable instruments and skillful observers, in order to provide for a systematic study of nebular changes. He has made preparations accordingly at Mendon, and he proposes to construct, upon a large scale, a telescope similar to the one with a very short focus with which he obtained in 1871 a very luminous spectrum of the corona.—*Comptes Rendus.*

An Aged Teacher.—Michel-Eugene Chevreul, although he has entered upon his ninety-fifth year, still continues his lectures at the museum of Natural History in Paris. He was elected a member of the Institute in 1825 and he is invariably in his seat at the weekly sittings of the Academy. He was appointed Professor of Chemistry in 1830, and for fifty years he has continued his instructions without having ever been required to provide a substitute. The manuscript of his programme for 1880 is written in a bold and beautiful hand. His figure is tall and imposing, his appearance still young and fresh looking, his manners genial, calm, gentle, and he always welcomes his visitors with a smile.—*Les Mondes.* C.

Nutricine.—E. Moride has prepared a new elementary substance which he calls nutricine. He combines raw flesh with other nitrogenous food which absorbs the juices of the flesh and, perhaps, forms with them some organic combinations which are, as yet, undetermined. He dries the whole in the air or in a stove moderately heated, then pulverizes and sifts it. The powder is of a fine gray or yellowish color and of an agreeable taste. It may be solidified by gum water, albumen or grease, so as to form tablets, cylinders and cubes of various weight, which can be divided, as needed, for making soups, sauces or biscuits. The nutricine contains all the elements of the flesh in their natural condition; even the blood preserves all its properties of solubility, coloration and coagulation under the influence of heat. It is more nitrogenous and more nourishing, for equal weights, than meat itself, because all the worthless portions of the meat are rejected and the fluids are replaced by farinaceous substances, which contain some additional amount of nitrogen. The same system, when applied to the blood or meat of horses and the refuse of abattoirs, gives a useful food for dogs, hogs, chickens and ducks.—*Comptes Rendus.*

The Atlantic Cable and Hughes Telegraph.—By using the Tomasi relay the Hughes telegraph has been able to print messages which were transmitted through resistances far greater than those of the Atlantic cable, and Abbé Moigno ventures the prediction that the experiments will soon be successfully repeated with the cable itself.—*Les Mondes.* C.

Paste for Paper.—To ten parts by weight of gum arabic add three parts of sugar in order to prevent the gum from cracking; then add water until the desired consistency is obtained. If a very strong paste is required add a quantity of flour equal in weight to the gum, without boiling the mixture. The paste improves in strength when it begins to ferment.—*Chron. Industr.* C.

Burnt Steel.—Mere heat does not harm steel or iron: they may be heated and cooled an unlimited number of times, provided they are not allowed to come in contact with the air so as to absorb oxygen. In heating a piece of steel the amount of blast has much more to do with the burning than the heat. If the extra oxygen is taken out of the burnt steel it can be made to work just as well as it did before. The proof that heat does not harm steel is found in the fact that if a piece of steel is put in a closed box, and luted up so as to keep out the air, it can be heated and cooled an unlimited number of times without injury.—*L'Ingen. Univ.* C.

Light and Electricity.—In 1873 W. C. Röntgen was led by observing that a glass plate which had been fractured by the electric spark became doubly refracting, to inquire whether a similar influence might be exerted by electricity without fracture. Kerr, Gordon and Mackenzie subsequently published a series of experiments upon the subject, none of which seemed very conclusive until Kerr's communications appeared in the *Philosophical Magazine* for 1879. Röntgen was then induced to experiment with various substances, and he found that transmitted light undergoes changes through electric influences which are precisely similar to those of ordinary double refraction. The intensity varies in the electrical field with the electrical force, and it increases with the difference of potential between the electrodes. By these experiments he has succeeded in thoroughly confirming the classification of fluids as positive and negative.—*Ann. der Phys. und Chem.* C.

American Sewing Machines. — The value of the sewing machines exported last year was as follows: Germany, $539,000; Great Britain, $481,000; Mexico, $153,000; Australia, $110,000; Colombia, $83,000; Cuba, $66,000; France, $41,000; South America, $84,000; Central America, $12,000, and other countries $82,000, making a tatal of $1,661,000.—*Fortschr. der Zeit.* C.

Photographing the Chromosphere.—Janssen has been induced, by his late novel experiments, to undertake photographs of the chromosphere. He allows the solar luminous action to continue so long that the solar image becomes positive to the very circumference, without going beyond it. The chromosphere is then shown in the form of a dark ring, with the thickness of 8″ or 10″. He has compared positive and negative solar photographs, which were obtained on the same day and with the same instrument; the measurement of the diameters shows that the dark ring in question is wholly outside of the solar disk. *Comptes Rendus.* C.

Nervous Velocity in the Lobster.—Frédéricq and Vaudevelse have been experimenting upon the velocity of transmission of the motive excitement in the nerves of the claw of the lobster. They used the graphic method, which was employed by Helmholtz in his researches upon the propagation of the nervous motor influences in the frog. In the winter experiments, at Ghent, with a temperature of 10° to 12°C. (50° to 53·6°F.) they found a velocity of about 20 feet per second. In the summer experiments, at Roscoff, with a temperature of from 18° to 20°C. (62·4° to 68°F.) they found velocities of from 30 to 40 feet per second.—*Comptes Rendus.* C.

Thermal Theory of the Galvanic Current.—J. L. Hoorweg lays down the following laws: *a.* When two conductors come into contact a warm current produces a development of electricity, hence arises a constant electrical difference between the two substances. *b.* If the sum of the potential differences in a closed circuit varies from zero a continuous electrical current arises in the circuit. *c.* This current exists at the expense of the heat in one of the points of contact, and it develops heat at the other point. *d.* All voltaic currents are thermal currents. *e.* The chemical action in the battery and the decomposition are consequences of the galvanic current.—*Ann. der Phys. und Chem.* C.

Advantages of Steel.—Dr. Siemens, whose thorough competence is universally known, claims that in every case when strength and magnitude are both required the use of steel is without a rival. He asserts that even for an ordinary house steel gives more security than wood, is six to eight times as strong, and costs less. He thinks that before many years elapse we shall see steel introduced into buildings of all kinds, and that it will gradually supplant iron, in the same way that iron already tends to take the place of wood.—*L'Echo Industr.* C.

Relations between Chemical Mass and Heat of Combination.—Berthelot finds that the elements which belong to any group, when they unite with any given simple body in order to form comparable compounds, set free quantities of heat which vary inversely with the chemical mass; the stability of the compound decreases in the same ratio. The decrease sometimes extends even to the change of volume which is produced by the combination of solid elements, when they form a solid compound. We are thus enabled to investigate the mechanical significance of the various relations.—*Comptes Rendus.* C.

Book Notices.

A Course on the Stresses in Bridge and Roof Trusses, Arched Ribs and Suspension Bridges. Prepared for the Department of Civil Engineering at the Rensselaer Polytechnic Institute. By William H. Burr, C.E. 8vo. New York: John Wiley & Sons, 1880.

Any one who has been through that Institute can realize the advantage given to the student by a work of this kind, a text-book prepared by his own instructor, adapted by him to the course which he teaches, arranged according to the methods and nomenclature of that instructor and considering just those subjects which he feels to be most important for the present wants of his pupil. This was particularly the case in the writer's day, when the lecture system was so largely used, and a work of this kind, especially at a time when few, if any, books on the higher branches of engineering were published or accessible, would have been, indeed, a great boon. How far this want now obtains, and

what methods are used at the Institute, the writer is unable to say, but he has no doubt that even with the host of reliable text-books within reach of the student, the present work will still supply a deficiency, particularly at that school, and also outside of it.

The writer is well aware that many practical details, important and essential to the engineer, are not treated of in works of this kind, and it would be better if they could be. Unfortunately, however, the professors of our institutions seldom have the practical knowledge necessary to so treat them. The compensations paid are not usually sufficient to enable a man highly skilled in practical attainments by many years of service to accept such a position, as he can do better in the active practice of his profession outside. On the other hand, the practical man has seldom the time, or that inclination to literary pursuits, requisite to produce a good book. His practical experience, too, is part of his "stock in trade," and he does not always care to part with it. Our engineering societies are helping very much in this matter, encouraging the preparation and reading of papers, and must be mainly relied upon to further this branch of the subject.

All that the student can do in his four years' course at college, and all that is expected, is that he shall advance so far in his theoretical studies, with such practical information as it is possible to give him, to put him in a position to continue his work of gaining knowledge by himself after his college days are over, and to enable him to take a low position in actual service, where he can gain the practical part of his profession, working it up in connection with the theoretical, applying the latter wherever he can, and gaining for himself a place in the world. It would be impossible in four years to gather in all the instruction necessary to turn out a full-fledged practical engineer. It is a great mistake to think so, and although our young graduate comes out sometimes with this idea, the sooner his mind is disabused of it the better for himself and for others. The best student who ever graduated, when he leaves college is nearly at the foot of the ladder.

We are led to these remarks by reading a criticism on this work from another paper, and we want to say that we believe this book has been prepared by the author for *students*, particularly those of the Rensselaer Polytechnic Institute, not for the man who wishes to learn the *practical* part of his profession.

The theory as a usual thing *must* be taken up at the student time of life, when the brain is clear and active, unoppressed with cares of a

professional business. Let the student get what practical part he can; do not deny it to him, but do not make a sacrifice of theory for it.

J. M. W.

THEORY OF SOLID AND BRACED ELASTIC ARCHES; applied to Arched Bridges and Roofs in Iron, Wood, Concrete or other Material. Graphical Analysis. By Wm. Cain, C.E. 18mo. New York: D. Van Nostrand, 1879.

Another one of those useful little books comprising the Van Nostrand science series is now before us.

This is the third volume which Mr. Cain has issued on the subject of arches. The others, particularly that on voussoir arches, have been employed by the writer in practical work for some time, and he can testify to their usefulness. The principles involved in the discussions contained in the present volume are believed to be entirely correct, and the results are in such a form as to be practically available by the engineer in any operations concerning arches which may occur in his business operations, matters that until within the last few years have been difficult of solution without much labor, and even then, perhaps, involving more or less uncertainty.

The writer well remembers the pleasure with which he perused Mr. Baker's paper on arches some years ago, followed afterwards by Bell, in his communication to the Institution of Civil Engineers, whose method was a great advance on anything hitherto accessible. He feels sure that engineers will welcome Mr. Cain's contribution, as adding considerably to our knowledge on this interesting subject. J. M. W.

DWELLING HOUSES: THEIR SANITARY CONSTRUCTION AND ARRANGEMENTS. By Prof. W. H. Cornfield, M.A., M.D. No. 50 of Van Nostrand's Science Series. New York: D. Van Nostrand. 1880.

The work before us consists in the reprint from *Van Nostrand's Magazine* of a series of lectures upon a subject which ranks among the most practical of those which press themselves upon our consideration; already dignified as a science, although a growth of but thirty or forty years at the most, it claims among its votaries men of talent and learning, often attracted by the opportunities it offers for exercise of their capabilities for usefulness in a sphere of such unquestioned practical importance. The prevalence of serious diseases which scientific dis-

covery has associated with filth in its various forms, has tended to enlist the interest of all classes, and in direct proportion to their enlightenment, even the sordid and mercenary are compelled to recognize its importance from a money standpoint. And while it may be, as is too often the case, that the hobby is saddled with more than its legitimate burthen, nevertheless the agitation of the matter will be the surest method of arriving at the truth, which may be said in the present case to often literally lie "at the bottom of a well." The following paragragh is so suggestive of the practical nature of our subject it requires no comment:

"It has been clearly shown that the dampness of the soil under the houses is one of the great factors in the production of consumption. Dr. George Buchanan (see ninth report of the Medical Office of the Privy Council) demonstrated that in every instance where the level of the subsoil water in a town has been lowered, that is to say, where the distances between the basements of the houses and the level of the water in the soil had been made greater, the death rate from consumption had decreased, in one instance to the extent of not less than fifty per cent."

Attention is also directed to the evil that is known as "made ground" in the neighborhood of towns, consisting of rubbish of various kinds used to raise the surface to the proper grade; we do not think, however, that our author is emphatic enough in his protest against what we regard as an unjustifiable disregard of the simplest sanitary knowledge, for a large element in this rubbish consists of tinman's scraps, which with ashes, etc., create a porous and unstable foundation, liable to settle for a long period after being built upon, and in an irregular manner, thus disturbing the foundations, while providing facilities for the formation of foul emanations.

Attention is also directed to the all-important fact, that water which does not indicate impurity of any kind to the senses may yet contain injurious substances in solution, which can only be discovered by chemical analysis, although they may often be suspected from its history. While the work is pregnant with important suggestions, the principal fault lies in the attempt to illustrate without cuts which, in a popular publication, is unsatisfactory, as it presumes an ability of discernment which the average reader does not possess; there is, however, much which we commend to the perusal of all who desire to take nformation in homœopathic doses and agreeable forms. W. B. C.

Franklin Institute.

HALL OF THE INSTITUTE, December 15th, 1880.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 160 members and 35 visitors.

The minutes of the last meeting were read and approved.

The President announced that nominations for officers to serve the ensuing year were, in accordance with Article XIV, Section 7, of the By-Laws, the privileged business of the meeting, and after the reading of the names of those whose terms had expired he asked for nominations.

Mr. Cartwright nominated the retiring members of the Board.

The following members were placed in nomination:

For President, William P. Tatham.

Vice-President, Charles Bullock.

Secretary, Dr. Isaac Norris.

Treasurer, Frederick Fraley.

Managers, Washington Jones, Pliny E. Chase, Joseph M. Wilson, Theodore D. Rand, Coleman Sellers, Charles S. Heller, Frederick Graff, W. L. Du Bois, H. P. M. Birkinbine, George V. Cresson, Henry G. Morris, Robert Briggs, A. E. Outerbridge, Jr., Henry Seybert, Dr. Charles M. Cresson.

For Auditors, W. B. Cooper and Louis S. Ware.

Representative in Pennsylvania Museum and School of Industrial Art. No nomination made.

The Actuary presented the minutes of the Board of Managers, and announced that 19 persons were elected members of the Institute at their last meeting.

The Secretary reported the following donations to the Library:

Publication Industrielle des Machines, Outils et Appareils les plus perfectionnés, etc. Par Armengaud ainé. Vols. 1 to 10 of text and Vols. 1 to 10 of plates. From Fred'k Graff, Philadelphia.

Steam Boilers, their Design, Construction and Management. By W. H. Shock. From B. H. Bartol, Philadelphia.

Proseingamento del Lago Fucino Eseguito dal principe. D. A. Torlonia. Firenze, 1871. From Fred'k Graff.

Reports from the Consuls of the United States on the Commerce, Manufactures, etc., of their Circular Districts. No. 1. Oct., 1880. From the State Department, Washington.

Les Tarifs des Chemins de Fer et l'autorité de l'Etat. Par M. Léon Aucoq. From the Author, Paris.

Annual Report of the Surgeon General, U. S. A., for 1880. From the Surgeon General.

United States Coast and Geodetic Survey. Appendix 13 to Report for 1877. From the Survey Office, Washington.

American Ephemeris and Nautical Almanac for 1883. From the Bureau of Navigation, Washington.

Report of the Commissioner of Fish and Fisheries for 1878. From the Commissioner, Washington.

Annual Report of the Secretary of War for 1880. From the Secretary, Washington.

The Steam Engine Familiarly Explained and Illustrated. By D. Lardner. From W. P. Tatham, Philadelphia.

Journal of the Royal Geographical Society. Vol. 49. 1879. From the Society, London.

Annual Reports of the Adjutant General of Pennsylvania for 1864 and 1874. From the Adjutant General, Harrisburg.

The Northwestern Miller. January to August, 1880. From the Publishers, Minneapolis.

Second Geological Survey of Pennsylvania. Reports C^3, C^3 Maps; G^2, G^3; I^3; O^2; R; T; V^2.
From the Survey Office, Harrisburg.

Mr. W. Barnet Le Van read the paper announced for the evening, entitled "Ninety Miles in Sixty Minutes, or How to Accomplish the Distance between New York and Philadelphia in One Hour."

The paper was a continuation of one read by him, some time ago, on High Railway Speeds (see JOURNAL for July, 1880), and undertook to show the difficulties in the way of running trains at this high speed at present, and how they could be overcome. The subject was treated from a technical point of view, but some of the facts mentioned were of general interest. Thus, the distance in an air line between New York and Philadelphia was stated to be a fraction less than 81 miles (80·9), over a comparatively level country. The existing roads are far from being straight. On the Pennsylvania line, in the 88·4 miles between Philadelphia and Jersey City, there are 84 curves (fifteen in the fifteen miles between Germantown Junction and Schenck's Station). The greatest length of straight track between this

city and Trenton does not exceed three miles, and the greatest in the entire road does not exceed ten miles. The Pennsylvania Railroad is not responsible for this condition of the road, but has, in fact, done much to straighten the line, and, notwithstanding these drawbacks, runs trains on the road at the rate of 50 miles per hour. On the Bound Brook route there are forty-three curves, one on the bridge crossing the Delaware river, which has a radius of 2865 feet, is 1837 feet long and ascends 19 feet to the mile. The greatest stretch of straight track is from Skillman's east—14 miles. To compensate for the centrifugal force tending to throw the cars from the track when running at high speeds on curves the outer rail has to be raised. On a curve of three degrees radius the super-elevation required on a gauge of fifty-six inches, while less than five inches at fifty miles an hour, would have to be *sixteen* inches at ninety miles per hour. On the Pennsylvania road the super-elevation is one inch for each degree of curvature up to five inches, which is the limit. The speed must be reduced beyond that to correspond with the curve. This is one of the limitations put upon high speeds on existing roadways. Mr. Le Van considered others at great length, and summed up by saying that, after a careful study of the subject, he was satisfied that a paying road could be built to run in a straight line between New York and Philadelphia, reducing the distance about ten miles, and enabling trains to be run through in sixty minutes. One of the means of effecting this purpose would be a reduction of the dead-weight in the trains. The fast trains now running between this city and New York have generally four cars with engine and tender, weighing 232,000 pounds, or 116 tons dead load, and are 264 feet long. For from four to eight tons of passengers carried, trains are made up weighing from 110 to 150 tons.

Mr. Le Van thought it would pay to build a line so perfect in all its details as to exclude rival lines, and attract to itself all the through business. The line he pictured crossed no roads at grade, and had only two curves of 10,000 feet radius each.

Mr. Nystrom said that there were two distinct problems in Mr. Le Van's project—one geometrical, the other dynamical. The geometrical problem was as to whether a straight line was the shortest between two points, an axiom that does not seem to have been recognized by railroad engineers. He did not think the dynamical problem difficult to solve. There would be no difficulty in going to New York in even less time than Mr. Le Van proposed, so far as driving the engine was

concerned; the chief difficulty is in getting roadway strong enough and stable enough to stand the pressure.

A curiously constructed high speed rotary engine was shown, the invention of Mr. Tegnander, of Gothenburg, Sweden. It is a double-acting engine, the cylinder having four chambers and pistons, and when the steam enters the chamber it acts on the bottom and top of each alternate piston. The piston rods are connected through ball bearing with a circular plate keyed on the engine shaft, and when the latter is made to revolve by the steam pressure its rotary motion is transmitted to the cylinder and pistons, so that every part of the engine is kept revolving except the slide and steam chest. The inventor claims that the engine is balanced with great nicety, so that the weight of the working parts is concentrated on the central line of the frame, and, consequently, there is very little vibration. The engine is comparatively small, easily managed, and the inventor claims that it is economical, and will give high speed with a minimum of wear and tear.

In answer to questions as to power of the engine, cost, etc., Mr. Lofquist, who exhibited it, said that the engine had about $8\frac{1}{4}$ effective horse-power, and when tested here was running at the rate of 800 revolutions per minute, with 50 pounds of steam pressure, driving a dynamo-electric machine, and the packing had not been touched since the engine had been tried in Sweden. For every 5 pounds pressure per square inch, the engine working with one-third expansion, the increase of power was 0·9 horse-power. With 100 pounds pressure the effective horse-power was 15, when tested in Sweden. The price at which such engines can be sold has not been determined. This one, made in Sweden and constructed by itself, cost about $200.

Mr. Orr inquired whether there had been any test of its durability. The reply was that there had been no prolonged test. This engine had been in use for about eight months, and was perfectly steam tight.

Mr. Robert Briggs said that his anticipations about this engine had been more than realized. It was one of the prettiest specimens of this class of engines that he had ever seen. It was not absolutely new, but presented many features that were improvements upon older engines. It is economical in bed plate, its moving parts are arranged so that it runs quite independent of any fly wheel, and it is therefore very well suited for use in small yachts. The first disk engine was made for this purpose, but could not be kept steam tight,

although in the hands of some of the leading engineers of England. This engine of eight horse-power represents one that, of another type, would weigh six or eight times as much. There are three or four movements of the same class, and Mr. Briggs described some of them.

Mr. Jones inquired whether Mr. Briggs remembered a Boston engine, with four cylinders, designed to be used as a fire-engine, saying that it appeared to him to be very similar to this.

Mr. Briggs said he could not recall that particular engine, but had seen several that were similar in form and movement.

Mr. Tatham said that at least thirty-five years ago an inventor came to Philadelphia from Richmond, Va., with plans of an engine substantially the same as this, and having the cylinder set at an angle to the engine shaft, like this.

Mr. Eldridge inquired whether the movement was not just the reverse of a blower shown at the Institute some years ago. In regard to the engine described by Mr. Briggs, and of which he had made a drawing, Mr. Eldridge said that he had seen one like it at the Paris Exposition two years ago.

Mr. Briggs could not recall to memory the blower mentioned by Mr. Eldridge, but, continuing his remarks, said that the disk engine in England had proved a failure. It differed, however, in its details from this.

Prof. Marks said that this and the engine described by Mr. Briggs were of a very old type, that has been re-invented about every five years since the beginning of the century. A full description of many of them may be found under the head of Chamber Crank Trains, in Reuleaux's *Kinematics of Machinery*, translated by A. B. W. Kennedy.

In answer to further inquiries, it was stated that a 30 horse-power engine of this type had been built as well as the one exhibited, and several of smaller horse-power. A recent number of the *Teknisk Tidskrift* states that considerable interest is being taken in the invention by the Society of Civil Engineers of Stockholm.

The Secretary's report included an account of Daniel's Safety Lock, where, by a simple adjustment, an increased security is obtained without additional cost in its manufacture, and after closing the door the mere operation of latching with the knob simultaneously bolts the lock, and throws a guard of chilled iron over the keyhole, preventing

its unlocking with the key or other implement. Another advantage claimed is that in the event of fire or other cause for alarm, the operation of opening by the knob from the inside at the same time unlocks the door, and the loss of the key, therefore, does not impair the efficiency of the contrivance, while extra bolts, chains, etc., on the door are done away, the mechanism being very simple and, it is thought, durable in action.

Attention was called to Lance's Wood and Rubber Step Pads, adjustable to any step; Haedrich's Improved Horse Sandal, and the letter of endorsement from Henry Bergh, highly recommending the invention; Campbell & Co.'s Horse Sandal, and Evans' Heat Deflector and Gas Escapement Damper, in which the cone-shaped deflector is attached to the centre of a duplex iron valve, capable of being revolved and opened or closed at will.

Reports from the Committees on a New Building and upon Prof. Frazer's resolution with regard to Expert Testimony were read.

Mr. W. B. Cooper's amendment to Article V, Section 2, of the By-Laws, that "no vote shall be cast by proxy," was adopted.

Mr. Theodore D. Rand's resolution, asking the Pennsylvania Legislature to continue the appropriation to the Geological Survey of the State, was, on motion, adopted.

Mr. Hector Orr's resolution, asking Congress to adopt Electric Lighting for Washington, was defeated.

Mr. Albert G. Buzby's resolution, asking for a committee of five members to report at the next meeting whether the usefulness of the Institute as a promoter of the Mechanic Arts could not be increased by a re-organization of the Committee on Science and the Arts, and changes in its methods of granting awards, etc., was adopted.

The Secretary announced the deaths of Mr. George R. Barker and Dr. Alexander Wilcocks, and asked that committees might be appointed to prepare suitable memorials, which was adopted.

Mr. Cartwright moved that the Secretary be instructed to issue a printed card containing the dates of Stated Meetings, the names of Standing Committees and other information of interest to members, and to advertise the papers to be read and other business to be brought forward in a morning and an evening paper, in place of the postal cards sent to the members, but it was not agreed to.

Upon motion, the Institute adjourned.

Isaac Norris, M.D., *Secretary.*

JOURNAL

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On the EXPERIMENTS with the PERKINS MACHINERY of the STEAM YACHT "ANTHRACITE."

By Chief-Engineer Isherwood, United States Navy.

(Continued from page 17.)

Experiment made at the New York Navy Yard with the Machinery of the Steam Yacht "Anthracite" to Ascertain its Economic Development of Power.

The following table contains the data and results of the experiment made by the Board of Naval Engineers on the machinery of the steam yacht *Anthracite* to ascertain its economic development of power, the vessel being secured stationary to the wharf of the New York navy yard. For facility of reference the quantities have been arranged in groups, and the description of each is so full that but little additional explanation is required.

The experiment continued, uninterruptedly, 23 hours and 58 minutes, during which all the conditions were maintained as uniformly as possible. The machinery throughout this time was operated by Mr. Perkins' engineers and fireman in the manner which their experience had taught them would produce the highest economic results.

The fuel was semi-bituminous coal from the Cumberland mines in Maryland, and was rather below the average quality. The experi-

ment was commenced with a clean fire in full action, after the machinery had been several hours in operation, and was ended with the fire in the same condition as regards cleanliness and thickness as nearly as could be judged by the eye. The coal was accurately weighed on shore into bags of uniform weight and delivered on board as required, the bunkers of the vessel having been previously sealed. The time of emptying each bag was noted in the steam log or engine room record. The fire was kept well cleaned, leveled, and free of holes; and all the refuse from the coal, in ash, clinker, soot and dust was carefully weighed in the dry state.

The quantity of feed water pumped into the boiler during the experiment was accurately measured in two iron tanks placed on the vessel's deck and filled alternately by the water of condensation delivered from the condenser by the air pump. At the end of the experiment the level of the water in the glass gauge of the boiler was made the same as at the beginning, and the quantity required for this purpose was measured in the tanks with the rest. The weight of water was calculated from the carefully measured contents in cubic feet of the tanks, the number of tankfuls emptied, and the weight of the water per cubic foot at the temperature it had in the tanks. The exact time at which each tankful was emptied was noted in the steam log.

The number of double strokes made by the pistons of the engine, or revolutions of the main shaft, were given by a counter worked by the engine, the number on the counter being entered half hourly in the steam log.

The steam pressure in the boiler and in the receiver was noted from gauges in the engine room, and entered half hourly in the steam log, as well as the vacuum in the condenser, and the height of the barometer. The throttle valve was carried wide open during the entire experiment. At half hourly intervals the temperatures were noted of the external atmosphere on deck, of the air in the engine room, of the injection or sea water, of the water discharged overboard by the circulating pump, of the feed water in the hot well, and of the feed water in the tanks. The following table contains the means of all these half hourly observations. The temperatures given in the table of the steam in the boiler, and in the first cylinder at the commencement of the stroke of its piston, considered as saturated in both cases, are cal-

culated from a formula deduced from the experiments of the French Academy and of the Franklin Institute.

During the experiment a complete set of indicator diagrams were taken every half hour, the means from all which are given in the following table. Four indicators were employed and they remained permanently in position, attached to the cylinders by short straight pipes. A set of diagrams comprised one from the top of the first cylinder, one from the bottom of the second cylinder, and two respectively from the top and bottom of the third cylinder. There were taken in all one hundred and ninety-two diagrams.

The point at which the steam was cut off in the first and third cylinders, respectively, was obtained from the diagrams and corresponds to the point of inflection between the throttled and expansion curves.

The pressures in the cylinders are the means given by all the diagrams, which were measured for the pressure at the commencement of the stroke of the piston, at the point of cutting off the steam, and at the end of the stroke of the piston, all above zero or the line of no pressure as given by the barometer. The mean back pressure against the pistons during their stroke, and the back pressure at the commencement of the stroke, are given, both above zero, the latter back pressure is the minimum in the cylinder. The indicated pressure, representing the mean ordinate of the diagrams, was obtained by a planimeter. The net pressure given in the table is the indicated pressure less two pounds per square inch of pistons allowed for overcoming the friction *per se* of the moving parts of the engine. What is called the total pressure on the piston is the sum of the indicated pressure upon, and of the mean back pressure against, the piston during its stroke.

Some explanation may be required as to the calculation of the various horses-power given in the table, and the uses made of them. Of course, the indicated and the net horses-power developed in the different cylinders are calculated from the entire areas of their respective pistons and for the indicated and net pressures upon them. The total horses-power developed in the first cylinder are also calculated in the same manner from the total pressure on the piston and from its entire area. The total horses-power developed in the second cylinder, while calculated for the total pressure upon its piston per square inch, are not calculated for the entire area of its piston, but only for the annular space remaining after the area of the piston of the first cylinder has

been deducted from the area of the piston of the second cylinder. In like manner, the total horses-power developed in the third cylinder is not calculated for the entire area of its piston, but for only the annular superficies left by the subtraction of the area of the piston of the second cylinder from the area of the piston of the third cylinder, and for the total pressure upon that superficies in pounds per square inch.

In the compound engine, the back pressure overcome by the piston of the small cylinder is more or less utilized upon or transferred to the piston of the large cylinder, where, for a superficies equal to the area of the piston ot the small cylinder, it develops a pressure equal to the sum of the indicated and back pressures per square inch upon the piston of the large cylinder. Thus a portion of the indicated horses-power developed *in* the large cylinder is really developed *by* the piston of the small cylinder. The only portion of the indicated horses-power developed *in* the large cylinder that is developed *by* its piston is what is due to the remainder of the area of that piston after subtraction of the area of the piston of the small cylinder. And the total horses-power developed by the piston of the large cylinder is what is due to the annular superficies left by the above subtraction and to the total pressure upon it composed of the sum of the indicated and back pressures upon the piston of the large cylinder per square inch.

The total horses-power represents the entire dynamic effect produced by the steam, and, in the compound engine, as in the simple engine, would be the sum of the indicated horses-power developed in the small cylinder and in the large cylinder, and of the horses-power required to overcome the back pressure against the piston of the large cylinder, the latter power being calculated for the entire area of the piston of the large cylinder and for the back pressure against it in pounds per square inch above zero, were there no loss of pressure due to the transfer of the steam from the small to the large cylinder, but as there always is such loss it amounts to its value of back pressure against the piston of the small cylinder not utilized as pressure upon the piston of the large cylinder. With the same back pressure against the piston of the large cylinder of the compound engine as against the piston of the simple engine, and with the sum of the indicated pressures on the pistons of the compound engine reduced to the area of its large piston, equal to the indicated pressure on the piston of the simple engine, there will always be a less proportion of the total pressure utilized in

the compound than in the simple engine. In the compound engine, then, the total horses-power is the sum of the indicated horses-power and the horses-power required to overcome the back pressures against the pistons, calculating the latter power separately for each piston. It is essential to know, not only the total horses-power developed by the engine as a whole, but the fractions of that power developed by the piston of the small cylinder and by the piston of the large cylinder; because, in calculating the weight of steam accounted for by the indicator at any point of either piston after the closing of the cut-off valve, it is necessary to know not only the pressure of steam at said point, but also the total horses-power which have been developed up to that point by the expanded steam alone, that is, by the steam after the closing of the cut-off valve, the weight of steam condensed in the cylinder to furnish the heat transmuted into the total horses-power developed by the expanded steam alone having to be added to the weight due to the pressure at the given point.

In calculating the weight of steam accounted for by the indicator at the point of cutting off the steam in the small cylinder nothing is to be added for condensation in the cylinder due to transmutation of heat into the total power developed up to that point, the heat for that purpose having been expended in the boiler during the generation of the steam. After the closing of the cut-off valve in the small cylinder all the steam thenceforth used to the end of the stroke of the piston of the large cylinder is expanded steam.

In calculating the weight of steam accounted for by the indicator at any point, from the steam pressure there present, allowance must be made for the weight of steam already in the clearance and steam passage of the cylinder when the boiler steam enters, as only the excess over this weight has been drawn from the boiler. The weight of steam in the clearance and steam passage present when the boiler steam enters is calculated from the back pressure against the piston at the commencement of its stroke, and not from the mean back pressure during its stroke. This is why the back pressure at the commencement of the stroke of the pistons is given separately in the table.

The economic results are given for the cost of the indicated, net and total horse-power in pounds of coal, in pounds of the combustible portion of the coal, in pounds of feed water, and in Fahrenheit units of heat consumed per hour. Of these, the latter alone is exact for comparison with the performances of other engines. The cost in pounds of feed

water does not include the difference in the total heats of steam of different pressures, and in the total heats of feed water of different temperatures, which the cost in units of heat does. The cost in fuel is variable with the quality of the fuel, with the rate of its combustion, with the kind and proportions of boiler in which it is consumed, and with the skill of the fireman; the fuel measure is therefore not a proper one for the economic efficiency of the engine, *per se*, as compared with that of other engines, for which purpose it furnishes but a rough approximation. The true engineering comparison is for the cost of the *total* horse-power in units of heat consumed per hour. For commercial purposes the comparison should be for the cost of the *net* horse-power in units of heat consumed per hour. The total horse-power representing the entire work done by the steam, both usefully and in overcoming prejudicial resistances within the machine itself, while the net horse-power represents only the work done at the crank pin, which is the useful work available for purposes exterior to the machine itself. The number of Fahrenheit units of heat expended per hour per indicated, net and total horse-power developed by the engine, is the product of the multiplication respectively of the pounds of feed water consumed per hour per each of the said horse-powers by the number of units of heat put into that water, namely, the difference between the total heat of the boiler steam and the total heat of the feed water.

If the object be to compare the economic efficiency of two systems of machinery, as a whole, including engine, boiler, method of using the steam, etc., then the cost of the *net* horse-power developed in pounds of fuel consumed per hour is the proper measure, taking care that the fuel used is the same in both cases.

In the various calculations of the quantities in the following table allowance has been made for the different specific heats of water at different temperatures and under different pressures. Also for the different total and latent heats of steam of different pressures. All the steam in the cylinders was in the saturated state, as appears from the difference in the weight of steam accounted for at any point by the indicator and the weight drawn from the boiler.

All the various quantities observed during the experiment were carefully noted personally by experienced assistant engineers of the Navy under the supervision of the Chief Engineers composing the Board. The indicator diagrams were taken by these assistants with every precaution, the indicators having been previously tested, as were

also the scales used for weighing the coal and its refuse. Every attention was given to have the results minutely accurate. The cylinders, on trial, showed no leakage, and it is believed the entire machinery was in excellent condition.

Table containing the Data and Results of the Experiment made at the New York Navy Yard on the Machinery of the Steam Yacht ANTHRACITE *by a Board of United States Naval Engineers.*

	Date of experiment (vessel secured stationary to wharf),	August 13, & 14, 1880.
	Number of sets of indicator diagrams, taken half-hourly,	48·
TOTAL QUANTITIES.	Duration of the experiment in hours and minutes, consecutively,	23 58
	Total number of pounds consumed of Cumberland semi-bituminous coal,	4400·
	Total number of pounds of refuse in ash, clinker, etc., from the coal,	776·
	Total number of pounds of combustible (gasifiable portion of the coal) consumed,	3624·
	Per centum of the coal in refuse of ash, clinker, etc.,	17·6363
	Total number of pounds of feed water pumped into the boiler,	35114·
	Total number of double strokes made by the pistons of the engine,	148154·
ENGINE.	Steam pressure in the boiler, in pounds per square inch above the atmosphere,	316·50
	Steam pressure in the receiver, in pounds per square inch above the atmosphere,	10·54
	Position of the throttle valve,	Wide open.
	Fraction completed of the stroke of the piston of the 1st cylinder when the steam was cut off,	0·5206
	Fraction completed of the stroke of the piston of the 3d cylinder when the steam was cut off,	0·2835
	Number of times the steam was expanded,	25·7098
	In none of the cylinders was the steam cushioned, nor was there either steam or exhaust lead.	
	Vacuum in the condenser, in inches of mercury.	26·75
	Height of the barometer, in inches of mercury,	30·023
	Back pressure in the condenser, in pounds per square inch above zero,	1·6066
	Number of double strokes made per minute by the steam pistons,	103·02782

Temperatures.	Temperature, in degrees Fahrenheit, of the external atmosphere,	77·5
	Temperature, in degrees Fahrenheit, of the engine room,	98·5
	Temperature, in degrees Fahrenheit, of the injection, or sea water,	76·0
	Temperature, in degrees Fahrenheit, of the discharge water,	95·0
	Temperature, in degrees Fahrenheit, of the hot well,	122·0
	Temperature, in degrees Fahrenheit, of the feed water in the tanks,	120·5
	Temperature, in degrees Fahrenheit, of the steam in the boiler, considered as saturated,	420·0
	Temperature, in degrees Fahrenheit, of the steam in the 1st cylinder at the commencement of the stroke of the piston, considered as saturated,	385·0
Rate of Combustion.	Pounds of coal consumed per hour,	183·5883
	Pounds of combustible consumed per hour,	151·2101
	Pounds of coal consumed per hour per square foot of grate,	11·9869
	Pounds of combustible consumed per hour per square foot of grate,	9·8728
	Pounds of coal consumed per hour per square foot of outer heating surface,	0·6115
	Pounds of coal consumed per hour per square foot of inner heating surface,	0·8142
	Pounds of combustible consumed per hour per square foot of outer heating surface,	0·5036
	Pounds of combustible consumed per hour per square foot of inner heating surface,	0·6706
Steam Pressures in 1st Cylinder per Indicator.	Pressure on piston of 1st cylinder at commencement of its stroke, in pounds per square inch above zero,	201·64
	Pressure on piston of 1st cylinder at the point of cutting off the steam, in pounds per square inch above zero	169·12
	Pressure on piston of 1st cylinder at the end of its stroke, in pounds per square inch above zero,	91·50
	Mean back pressure against piston of 1st cylinder during its stroke, in pounds per square inch above zero,	45·95
	Back pressure against piston of 1st cylinder at commencement of its stroke, in pounds per square inch above zero,	36·57
	Indicated pressure on piston of 1st cylinder, in pounds per square inch,	110·98
	Net pressure on piston of 1st cylinder, in pounds per square inch,	108·98
	Total pressure on piston of 1st cylinder, in pounds per square inch above zero,	156·93

STEAM PRESSURES IN 2D CYLINDER PER INDICATOR.	Pressure on piston of 2d cylinder at commencement of its stroke, in pounds per square inch above zero, .	60·00
	Pressure on piston of 2d cylinder at the end of its stroke, in pounds per square inch above zero, .	31·30
	Mean back pressure against piston of 2d cylinder during its stroke, in pounds per square inch above zero, .	31·06
	Back pressure against piston of 2d cylinder at commencement of its stroke, in pounds per square inch above zero, ...	30·20
	Indicated pressure on piston of 2d cylinder, in pounds per square inch, ...	10·537
	Net pressure on piston of 2d cylinder, in pounds per square inch, ...	8·537
	Total pressure on piston of 2d cylinder, in pounds per square inch above zero, ...	41·597
STEAM PRESSURES ON 3D CYLINDER PER INDICATOR.	Pressure on piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero, .	27·71
	Pressure on piston of 3d cylinder at the point of cutting off the steam, in pounds per square inch above zero, ...	22·28
	Pressure on piston of 3d cylinder at the end of its stroke, in pounds per square inch above zero, .	9·55
	Mean back pressure against piston of 3d cylinder during its stroke, in pounds per square inch above zero, .	4·21
	Back pressure against piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero, ...	3·74
	Indicated pressure on piston of 3d cylinder, in pounds per square inch, ...	12·457
	Net pressure on piston of 3d cylinder, in pounds per square inch, ...	10·457
	Total pressure on piston of 3d cylinder, in pounds per square inch above zero, ...	16·667
BOILER VAPORIZATION.	No. of pounds of water that would have been vaporized in the boiler had the feed water been supplied at the temperature of 100 degrees, and vaporized under the atmospheric pressure of 29·92 inches of mercury, .	36509·6289
	No. of pounds of water that would have been vaporized in the boiler had the feed water been supplied at the temperature of 212 degrees, and vaporized under the atmospheric pressure of 29·92 inches of mercury, .	40775·4371
	Pounds of water vaporized from 100° Fahrenheit by one pound of coal, ...	8·2976
	Pounds of water vaporized from 100° Fahrenheit by one pound of combustible, ...	10·0744
	Pounds of water vaporized from 212° Fahrenheit by one pound of coal, ...	9·2671
	Pounds of water vaporized from 212° Fahrenheit by one pound of combustible, ...	11·2515

Horses-power.	Indicated horses-power developed in the 1st cylinder,	20·4308
	Indicated horses-power developed in the 2d cylinder,	7·8290
	Indicated horses-power developed in the 3d cylinder,	39·4483
	Aggregate indicated horses-power developed in all the three cylinders,	67·7081
	Net horses-power developed in the 1st cylinder,	20·0628
	Net horses-power developed in the 2d cylinder,	6·3430
	Net horses-power developed in the 3d cylinder,	33·1150
	Aggregate net horses-power developed in all the three cylinders,	59·5208
	Total horses-power developed in the 1st cylinder,	28·8902
	Total horses-power developed in the 2d cylinder,	23·2490
	Total horses-power developed in the 3d cylinder,	28·0133
	Aggregate total horses-power developed in all three cylinders,	80·1525
	Total horses-power developed by the expanded steam alone in the 1st cylinder,	10·7400
	Total horses-power developed by the expanded steam alone in the 2d cylinder,	23·2490
	Total horses-power developed by the expanded steam alone in the 3d cylinder,	28·0133
Weight of Steam accounted for by the Indicator.	Pounds of steam present per hour in the 1st cylinder at the point of cutting off the steam, calculated from the pressure there.	633·5094
	Pounds of steam present per hour in the 1st cylinder at the end of the stroke of its piston, calculated from the pressure there,	609·6268
	Pounds of steam condensed per hour in the 1st cylinder to furnish the heat transmuted into the total horses-power developed in that cylinder by the expanded steam alone,	30·8817
	Sum of the two immediately preceding quantities.	640·5085
	Pounds of steam present per hour in the 2d cylinder at the end of the stroke of its piston, calculated for the pressure there,	808·3992
	Pounds of steam condensed per hour in the 1st and 2d cylinders to furnish the heat transmuted into the total horses-power developed in those cylinders by the expanded steam alone,	93·9542
	Sum of the two immediately preceding quantities,	902·3534
	Pounds of steam present per hour in the 3d cylinder at the end of the stroke of its piston, calculated for the pressure there,	1150·7924
	Pounds of steam condensed per hour in the 1st, 2d and 3d cylinders to furnish the heat transmuted into the total horses-power developed in those cylinders by the expanded steam alone,	167·0720
	Sum of the two immediately preceding quantities,	1317·8644

Economic Results.	Pounds of coal consumed per hour per indicated horse-power,	2·7115
	Pounds of coal consumed per hour per net horse-power,	3·0844
	Pounds of coal consumed per hour per total horse-power,	2·3319
	Pounds of combustible consumed per hour per indicated horse-power,	2·2333
	Pounds of combustible consumed per hour per net horse-power,	2·5405
	Pounds of combustible consumed per hour per total horse-power,	1·9207
	Pounds of feed water consumed per hour per indicated horse-power,	21·6387
	Pounds of feed water consumed per hour per net horse-power,	24·6152
	Pounds of feed water consumed per hour per total horse-power,	18·2791
	Fahrenheit units of heat consumed per hour per indicated horse-power,	24265·69
	Fahrenheit units of heat consumed per hour per net horse-power,	27603·52
	Fahrenheit units of heat consumed per hour per total horse-power,	20498·22
Per Centum of Total Pressure on Pistons utilized as Indicated and as Net Pressures.	Mean indicated pressure on the piston of the 3d cylinder, equivalent to the sum of the indicated pressure on that piston and of the indicated pressures on the pistons of the 2d and 1st cylinders, reduced respectively in the ratio of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and for the fact of the 2d and 1st cylinders being single acting while the 3d cylinder is double acting, in pounds per square inch,	21·381
	Mean total pressure which applied to the piston of the 3d cylinder would produce the total horses-power developed by the engine, provided the indicated pressure on that piston was the above 21·381 pounds per square inch,	25·591
	Per centum of the mean total pressure on the pistons of the three cylinders utilized as indicated pressure,	83·55
	Mean net pressure on the piston of the 3d cylinder equivalent to the sum of the net pressure on that piston and of the net pressures on the pistons of the 2d and 1st cylinders reduced respectively in the ratio of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and with allowance for the fact of the 2d and 1st cylinders being single acting while the 3d cylinder is double acting, in pounds per square inch,	18·795
	Per centum of the mean total pressure on the pistons of the three cylinders utilized as net pressure,	73·44

DIFFERENCE BETWEEN THE WEIGHT OF WATER VAPORIZED IN THE BOILER AND THE WEIGHT OF STEAM ACCOUNTED FOR BY THE INDICATOR.

Difference, in pounds per hour, between the weight of water (1465·11822 pounds) vaporized in the boiler and the weight of steam accounted for by the indicator, in the 1st cylinder at the point of cutting off the steam,	831·6088
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 1st cylinder at the point of cutting off the steam, . .	56·76
Difference, in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator, in the 1st cylinder at the end of the stroke of its piston, . . .	824·6097
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 1st cylinder at the end of the stroke of its piston, . .	56·22
Difference, in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 2d cylinder at the end of the stroke of its piston, . . .	562·7648
Difference, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 2d cylinder at the end of the stroke of its piston, . .	38·41
Difference, in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 3d cylinder at the end of the stroke of its piston, . . .	147·2538
Difierence, in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 3d cylinder at the end of the stroke of its piston, . .	10·05

REMARKS.

Owing to the limited time the *Anthracite* could be placed under the command of the Board of Naval Engineers at the New York Navy Yard, the trial of her machinery was restricted to one experiment, and, consequently, to one set of conditions; but the soundness of the system can be judged only from a number of experiments made under widely varying conditions, and it was the intention of Chief Engineer Loring, had the vessel been long enough at the Navy Yard, to have made a series of experiments on her machinery, with varying boiler pressures and measures of expansion. He intended to have commenced with boiler steam of 75 pounds per square inch pressure above the atmosphere, and, keeping the same measure of expansion, to have

made a series of experiments with the boiler pressure increasing 25 pounds per square inch each time; after which, keeping the same boiler pressure, he would have varied the measure of expansion as much as possible, the object being to ascertain whether as good economic results could not be obtained with the lower boiler pressures and less measures of expansion as with the higher boiler pressures and greater measures of expansion, the practically important problem being to ascertain the limit, in both cases, at which increase of economy ceased.

Comparing the economic results obtained during the single experiment made, with those given by ordinary compound engines, it is evident that the latter, with one-fourth the boiler pressure and one-fourth the measure of expansion used with the machinery of the *Anthracite*, produce the power as economically. If the trials are to cease here, the inference follows that the enormous excess of boiler pressure and measure of expansion employed with the machinery of the *Anthracite* have failed to practically realize the economy theoretically predicable, leaving the grave inconveniences of such excess of pressure and expansion unbalanced by any advantage. With this result apparent, it becomes necessary to closely examine the conditions under which the experiment with the *Anthracite* was made.

The boiler pressure does not always represent the initial pressure on the piston, and still less does it represent the mean total pressure on the piston during the stroke; yet it is the latter on which the gain by the use of high pressure must be predicated. Notwithstanding the effect of any superheating which it may be practicable to give in the boiler, the steam in the cylinder is always found to be in the saturated state, that is to say, it has always the maximum density for its pressure, so that it is useless to generate steam at high boiler pressures with the expectation of realizing the economy of producing a given bulk of such pressure over an equivalent bulk of lower boiler pressure, if the former be used in the cylinder with the same mean total pressure on the piston as the latter. To make the mean total pressure on the piston equal in the two cases, the high boiler pressure must be used with a greater measure of expansion than the low boiler pressure, and the gain due to these different measures of expansion may be something or nothing, according to the conditions, but be it what it may, it is entirely distinct from the gain due to the use of steam of the high pressure, *per se*, so that if the high pressure boiler steam be correspondingly expanded in the cylinder to produce the same mean total

pressure in its piston as in the case of the low pressure boiler steam, the only gain due to the former will be what results from the higher measure of expansion with which it is used, and not from the higher boiler pressure. This important fact has been overlooked, and a gain under these conditions expected from the high pressure steam corresponding not only to its use with a higher measure of expansion, but also to its generation under a higher boiler pressure. The two cannot be had together. To ascertain what gain can practically be obtained from steam of higher boiler pressure, in function of pressure alone, the experiment must be made using it with the same measure of expansion as in the case of the steam of lower boiler pressure against which it may be tried. This was one of the determinations that Chief-Engineer Loring would have made could he have obtained the *Anthracite* for a sufficient length of time.

Now, during the single experiment with the *Anthracite's* machinery, the measure of expansion employed was so great (25·7098 times) that notwithstanding the high initial pressure on the piston of the first cylinder (201·64 pounds per square inch above zero), the mean total pressure above zero, referred to the piston of the third cylinder, was only 25·591 pounds per square inch above zero, so that in this case the *Anthracite's* engine had no advantage over the ordinary compound engine in function of higher mean total pressure on the piston.

It is quite probable, also, that the measures of expansion employed with ordinary compound engines even when using superheated steam, say from six to ten times, give as high an economy as can be obtained from greater measures, owing to the enormous cylinder refrigeration which attends the use of steam expansively, and which is *pro rata* to the total power developed by the expanded steam alone. If such be the fact, and all experiments thus far show it, then there is no reason to expect any higher economic results from the *Anthracite's* machinery for the conditions under which it was tried at the navy yard than were actually obtained. They were equal to those of an ordinary compound engine, and nothing more.

But it is quite possible that other conditions would have given a higher economy for the *Anthracite's* engine. Had, for instance, the same initial pressure on the piston of the first cylinder been maintained with the steam expanded, say six or ten times only, then, supposing nothing of economy to be lost by the decreased measure of expansion, there would have been the gain due to the resulting higher mean total

pressure on the piston in function of pressure only, and to the fact that the same final back pressure would have been a less per centum of the mean total pressure above zero; in other words, more of the mean total pressure on the piston would have been utilized as indicated, or as net pressure. Probably, however, the boiler could not supply the engine with the increased quantity of steam, in equal times, that these conditions would require, and, if it could, the rate of combustion of the coal would have to be largely increased, which would have been attended by a decreased economic vaporization, so that although an economic gain might have been obtained according to the water measure, or weight of steam consumed, yet a loss might have been experienced according to the coal measure. It is so impossible to change one condition in engineering without changing all or many, that it is unsafe to infer what would follow from a given change in one direction. Only the actual trial or appeal to Nature can be relied on each time.

It is true that any better results obtained with the water measure could be obtained with the coal measure also, by reducing the dimensions of the cylinders so as to develop the same power in each case with the same initial pressure on the piston. This, however, requires a new engine, and could not be realized with the machinery of the *Anthracite.*

Nor must the fact be omitted that with each increase in the initial pressure on the piston, the back pressure against it remaining constant, there results increase of cylinder condensation due to the increase in the difference between the temperature of the initial pressure and that of the back pressure. Each theoretical gain is attended with its inseparable practical loss, so that only a very small margin of difference seems possible in any case, and herein lies the true explanation of the failure, total or partial, of the many promising schemes and ingenious mechanisms for cheapening the cost of steam power in fuel.

In comparing the economic efficiency of steam of exceptionally high pressure and measure of expansion against that of steam of comparatively low pressure and measure of expansion, care must be taken that the degree of superheating be the same in both cases. Now, a considerable degree of superheating will increase the economy of any steam from 15 to 30 per centum according as the engine using it is a large or a small one, and superheating is as practicable with steam of one pressure as another. When a high degree of superheating is employed with high pressures and measures of expansion, discrimination must be made between what is due to it and what is due to them.

The great economic gain certain to follow the use of a high degree of superheating is a great temptation to employ it, especially for trials too short to develop its injurious effects on the metal of the cylinder and valves, and on the superheating apparatus itself. A prolonged use of any high degree of superheating has always been followed by such decrease in the durability and reliability of the machinery that, often as it has been attempted just so often has it been abandoned, consequently, notwithstanding the great economy given by it, it is in practical use nowhere. A very moderate degree of superheating is all that can be permanently maintained.

In the boiler of the *Anthracite*, extensive provision was made for highly superheating the steam, the superheating surface being twenty times the area of the grate surface and equal to the water heating surface, an excessive proportion, as compared with the five to eight times of the grate surface, which is usually given with large superheaters. The temperature of the gases of combustion in the chimney during the navy yard experiment averaged 700 degrees Fahrenheit, as obtained from the melting points of different metals suspended in the chimney; yet, notwithstanding the great extent of superheating surface and the high temperature of the gases of combustion upon it, the steam during the navy yard experiment did not appear to be much superheated. This can only be accounted for on the supposition that the steam was very wet when it went upon the superheating surface, owing to the priming or foaming of the boiler. It is greatly to be regretted that the temperatures of the steam on leaving the boiler and on entering the valve chest of the first cylinder were not taken during the navy yard experiment. This is the only important omission in the data; these temperatures would have shown the degree of superheating with certainty, whereas it can now only be inferred. The proper thermometers for the purpose were provided, but they were not inserted in position owing to the want of time to make the extensive connections required.

During the navy yard experiment the boiler was worked with the very moderate rate of combustion of about 12 pounds of coal per hour per square foot of grate surface, which was doubtless above its economic limit. All boilers of this description, that is, composed of small tubes in direct contact with the fire and connected continuously, the boiler not having a proper area of water surface for the disengagement of the steam, nor a proper capacity, and, more especially, proper height

of steam room for the separation by gravity of the carried up water from the steam, prime or foam violently when worked above a low rate of combustion. This result is inevitable, and such boilers are only fit for low rates of combustion, and even then require large proportions of water heating and steam superheating surfaces to give good economic vaporizations. As the rate of combustion is increased, so is the priming or foaming, and although the economic vaporization may up to a certain point be maintained by the conversion of the steam superheating into water heating surface by the priming, yet the superheating effect is thereby lost. If the rate of combustion be increased to 15, 20 or 25 pounds of coal per hour per square foot of grate surface the water will be driven by the heat entirely out of the lower tubes, and they will be quickly destroyed. A very great deal of this type of boiler is required in proportion to the power developed. It is neither durable nor reliable, particularly in ignorant or careless hands.

A proper test of this boiler requires it to be worked during a moderate time at the usual rate of combustion for steamers, say 20 pounds of coal per hour per square foot of grate surface. Its durability, its economic vaporization and its steam superheating would then become manifest; but experiments at rates of combustion considerably below what is necessary in practice only mislead.

The *Anthracite* has made two voyages across the Atlantic between Liverpool and New York, say 6500 geographical miles, and this was intended as a test and proof of the durability and reliability of her machinery. But the voyages were made at an exceedingly low rate of combustion, only about two indicated horses-power being developed for each square foot of grate surface, which evidently does not meet the objections, and leaves the problem just where it was. The fact at issue is: can the vessel without injury to her machinery make two consecutive voyages across the Atlantic at the rate of combustion employed by the ocean steamships running on the same track? The chimney of the *Anthracite* was so short that 12 pounds of semi-bituminous coal per hour per square foot of grate surface was as high a rate of combustion as could be permanently maintained. Had the vessel remained sufficiently long at the New York navy yard for exhaustive experimenting, her chimney would have been temporarily lengthened to about 55 feet above the grate, and all the coal burned that the draught would consume.

The great tendency of the boiler of the *Anthracite* to foam is evi-

denced by the fact that while the area of the steam port of the first cylinder is $\frac{1}{15}$th of the area of the piston, it was deemed prudent to make the cross area of the steam pipe less than $\frac{1}{47}$th of the area of the piston, thus providing for a great and permanent throttling of the steam beyond the control of the persons operating the engine. The extent of this throttling was such that whereas the boiler pressure during the experiment was 331·23 pounds per square inch above zero, the initial pressure on the piston of the first cylinder was 201·64 pounds, the throttle valve being wide open, the difference was 129·59 pounds.

That the steam had but little superheating when entering the first cylinder, probably not exceeding 50 degrees Fahrenheit, the most of which (35 degrees) was due to the throttling, appeared from all the indications. The packings of the valve stems and of the piston rods were unaffected; the steam when blown into the air did not look as though it was much superheated, and the condensation of steam in the first cylinder was enormous, exclusive of what was due to the development of the power. This condensation at the point of cutting off the steam in the first cylinder was 56·76 per centum of the entire weight generated in the boiler, and was maintained to the end of the stroke of the piston of that cylinder. In the second cylinder, owing to re-evaporation, this condensation fell at the end of the stroke of its piston to 38·41 per centum of the entire weight generated in the boiler, while at the end of the stroke of the piston of the third cylinder it fell further to 10·05 per centum by the continuance of the same cause.

This enormous cylinder refrigeration was due to the enormous measure of expansion (nearly twenty-six times) with which the steam was used, to the great difference between the temperature of the initial steam in the first cylinder and that of the back pressure steam in the third cylinder, and to the small dimensions of the cylinders whereby their inner surfaces, including the extensive surfaces of their disproportionately large steam passages, became very great relatively to the weight of steam contained. To counteract these cooling causes would require the steam to be very highly superheated, and provision for obtaining that effect was made by giving the boiler an enormous extent of superheating surface, but the necessary superheating was not obtained with the water carried at the experimental level, due doubtless to the priming or foaming of the boiler, notwithstanding the great throttling with which the steam was used.

A sensible proof of the enormous cylinder condensation in the case of the experiment on the *Anthracite* at the New York navy yard was

furnished whenever a set of indicator diagrams was taken. On opening the cock in the indicator pipe of the first cylinder a steady flow of water streamed from it, of the full area of the cock opening, and with considerable velocity, and continued unabated as long as the cock remained open, making the taking of diagrams very troublesome. When the cock in the indicator pipe of the second cylinder was opened, the same flow of water took place, and continued as long as the cock remained open, but the issuing quantity of water was much less and it flowed with much less velocity, the pressure being greatly less than in the first cylinder. When the cocks in the indicator pipes of the third cylinder were opened, no water came forth at any part of the stroke of the piston.

The engine during its working gave no indication of the enormous quantities of water of condensation passing through the cylinders; there was no slapping noise or noticeable jar, the entire mass of steam condensed during the steam stroke of the pistons being re-evaporated during the latter part of that stroke and the whole of the exhaust stroke under the lessening pressures by the heat contained in the water of condensation and in the metal of the cylinders on which this water rested.

The theoretical gain due to very high steam pressure worked in a cylinder with a low and constant back pressure, and to using it very expansively, is practically lost in greater refrigeration which inseparably attends both to a greater degree than with steam of lower pressure worked less expansively, and although the resulting condensation may be in a great measure prevented by previous superheating, yet if the same quantity of superheating be given to the lower pressure and less expanded steam, the latter would be proportionally benefitted, and the relative economy of the two perhaps not much changed.

It is of the greatest importance to mankind that there shall be discovered the pressure of steam, the measure of expansion, and the degree of superheating which will produce the highest economic results from the fuel, without prejudice to the durability and reliability of the machinery, and without requiring exceptional care and skill in its management. The attempt made to solve this problem by the construction of the machinery of the *Anthracite* is in the right direction, and most praiseworthy; and it will be a subject of lasting regret should a series of exhaustive experiments be not made with it. The mechanism is at hand, and all that is needed are the services of skilled and sagacious experimenters animated by the love of truth alone.

(To be continued.)

On the REVOLUTION of a FLUID ELLIPSOID with THREE UNEQUAL AXES.

By Dr. Thomas Craig, U. S. Coast and Geodetic Survey.

In 1834 Jacobi demonstrated the curious fact that the ellipsoid of three unequal axes is a possible form of equilibrium for a rotating mass of homogeneous fluid, provided the shortest axis of the ellipsoid be taken as the axis of rotation. The substance of Jacobi's investigation is given in the next two pages before proceeding to the particular form of the problem which it is desired to investigate.

If a, b, c denote the semi-axes of an ellipsoid we have for the equation of the surface

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1 \tag{1}$$

a, b, c are taken in order of magnitude. If the fluid mass is rotating around the axis of z with angular velocity ζ, we have for the equation of a level surface

$$\Omega + \frac{\zeta^2}{2}(x^2 + y^2) = C \tag{2}$$

where Ω is the potential of the whole mass at an internal point and C is a constant for each level surface, but varies in passing from one to another. Write

$$D = \sqrt{(a^2 + t)(b^2 + t)(c^2 + t)};$$

then for the potential of an ellipsoid whose semi-axes are a, b, c at an internal point x', y', z', we have

$$\Omega = -\pi\, a\, b\, c \int_0^\infty \frac{dt}{D}\left\{\frac{x'^2}{a^2+t} + \frac{y'^2}{b^2+t} + \frac{z'^2}{c^2+t} - 1\right\} \tag{3}$$

Substituting this in the equation of a level surface (2) and we have on dividing through by $\frac{3}{4}M$, where M is the mass of the ellipsoid,

$$\left.\begin{array}{c}\dfrac{2\zeta^2}{3M} - \displaystyle\int_0^\infty \frac{dt}{D(a^2+t)}x'^2 + \frac{2\zeta^2}{3M} - \int_0^\infty \frac{dt}{D(b^2+t)}\, y'^2 - \\ \displaystyle\int_0^\infty \frac{dt}{D(c^2+t)}\, z'^2 = C_1\end{array}\right\} \tag{4}$$

This equation must hold at the external surface of the ellipsoid which is given by (1) and, therefore, by comparison of these two equations we have

$$\frac{2\zeta^2}{3M} - \int_0^\infty \frac{dt}{D(a^2+t)} = \frac{k}{a^2}$$

$$\frac{2\zeta^2}{3M} - \int_0^\infty \frac{dt}{D(b^2+t)} = \frac{k}{b^2} \qquad (5)$$

$$- \int_0^\infty \frac{dt}{D(c^2+t)} = \frac{k}{c^2}$$

Subtract the second of these equations from the first and we have

$$a^2b^2 \int_0^\infty \frac{(a^2 - b^2)\, dt}{D(a^2+t)\,(b^2+t)} = k\,(b^2 - a^2); \qquad (6)$$

eliminating k by the third equation gives

$$(a^2 - b^2) \left\{ \int_0^\infty \frac{(a^2-b^2)\ dt}{D(a^2+t)\,(b^2+t)} - \int_0^\infty \frac{c^2\, dt}{D(c^2+t)} \right\} = 0 \qquad (7)$$

This is satisfied by $a = b$, which is the case of an ellipsoid of revolution round c; it is also satisfied by equating to zero the quantity in brackets

$$\int_0^\infty \frac{\left(a^2 + b^2) - \frac{a^2 b^2}{c^2}\right) t + t^2}{D^3}\, dt = 0 \qquad (8)$$

There can be no negative elements in this, unless

$$c < \frac{a\ b}{\sqrt{a^2+b^2}}.$$

Imagine a right-angled triangle with sides a, b. The perpendicular from the right angle to the hypothenuse is

$$\frac{a\ b}{\sqrt{a^2 + b^2}}.$$

From this it appears that c must be less than either a or b if the integral (8) is to vanish. If, however, c be very small, the integral will become negative. Therefore, there is some value of c which will satisfy the equation.* In looking over Clark's Geodesy and several

* Clark's Geodesy, pages 76 and 77.

papers by Liouville, Jacobi and others, I find no mention made of any solution of the problem of determining a possible form of equilibrium of the homogeneous fluid mass when the fluid no longer rotates round one axis but when there is rotation round three axes at right angles to each other. In what follows it is shown that the ellipsoid is a form of equilibrium of a fluid mass rotating in this manner. Equation (1) gives the ellipsoid whose semi-axes are a, b, c. If X, Y, Z are the components of the forces acting at the point x, y, z of the fluid mass we have

$$\frac{dp}{\rho} = X dx + Y dy + Z dz \tag{9}$$

and for the free surface of course

$$X dx + Y dy + Z dz = 0. \tag{10}$$

Let ξ, η, ζ denote the components of angular velocity round the axes x, y, z respectively; also, let Ω denote the potential of the mass at an internal point thus,

$$\Omega = -\pi\, a\, b\, c \int_0^\infty \frac{dt}{D} \left\{ \frac{x^2}{a^2+t} + \frac{y}{b^2+t} + \frac{z^2}{c^2+t} - 1 \right\} \tag{11}$$

or say,

$$\Omega = \text{const.} - \pi\, a\, b\, c\,(Ax^2 + By^2 + Cz^2) \tag{12}$$

where

$$\begin{aligned} A &= \int_0^\infty \frac{dt}{D(a^2+t)}, \\ B &= \int_0^\infty \frac{dt}{D(b^2+t)}, \\ C &= \int_0^\infty \frac{dt}{D(c^2+t)}. \end{aligned} \tag{13}$$

We have now

$$\begin{aligned} X &= \frac{d\Omega}{dx} + x\,(\eta^2+\zeta^2) \\ Y &= \frac{d\Omega}{dy} + y\,(\zeta^2+\xi^2), \\ Z &= \frac{d\Omega}{dz} + z\,(\xi^2+\eta^2). \end{aligned} \tag{14}$$

Substituting these in (9) we have for every surface of equal pressure and density

$$\Omega + \frac{x^2}{2}(\eta^2+\zeta^2) + \frac{y^2}{2}(\zeta^2+\xi^2) + \frac{z^2}{2}(\xi^2+\eta^2) = C \qquad (15)$$

Introducing the value of Ω and dividing through by $\frac{3}{4} M$ where M is the mass of the ellipsoid this becomes

$$\left\{\frac{2(\eta^2+\zeta^2)}{3M} - A\right\}x^2 + \left\{\frac{2(\zeta^2+\xi^2)}{3M} - B\right\}y^2 +$$

$$\left\{\frac{2(\xi^2+\eta^2)}{3M} - C\right\}z^2 = C^1 \qquad (16)$$

Comparing this with (1) we have

$$\begin{aligned} \frac{2(\eta^2+\zeta^2)}{3M} - A &= \frac{k}{a^2} \\ \frac{2(\zeta^2+\xi^2)}{3M} - B &= \frac{k}{b^2} \qquad (17) \\ \frac{2(\xi^2+\eta^2)}{3M} - C &= \frac{k}{c^2}. \end{aligned}$$

Subtracting the second of these from the first we have

$$\frac{2a^2b^2}{3M}(\eta^2-\zeta^2) + a^2b^2\int_0^\infty \frac{dt\,(a^2-b^2)}{D(a^2+t)\,(b^2+t)} = k\,(b^2-a^2) \qquad (18)$$

Eliminating k by means of the third equation and reducing

$$\frac{2(a^2-b^2)(\xi^2+\eta^2)}{3M} - \frac{2\frac{a^2b^2}{c^2}(\xi^2-\eta^2)}{3M} + (a^2-b^2)$$

$$\left\{\int_0^\infty \frac{dt}{D^3}\frac{\left(a^2+b^2-\frac{a^2b^2}{c^2}\right)t+t^2}{}\right\} = 0 \qquad (19)$$

Making $\xi = \eta = 0$ we have equation (8) which corresponds to the case of rotation round one axis. Equation (19) does not bring into evidence the angular velocity ζ and in consequence is not a form that can be advantageously employed; but dividing the first and second of equations (17) by the third we obtain two relations which are independent of k, and which contain all three of the quantities ξ, η, ζ: these are

$$\begin{aligned}\frac{2(\xi^2+\zeta^2)-3MB}{2(\xi^2+\eta^2)-3MC}&=\frac{c^2}{b^2}\\ \frac{2(\eta^2+\zeta^2)-3MA}{2(\xi^2+\eta^2)-3MC}&=\frac{c^2}{a^2}\end{aligned}\qquad(20)$$

Clear these of fractions, add and subtract $\xi^2+\zeta^2$ on the right side of the first and $\xi^2+\zeta^2$ on the right hand side of the second equation;

$$\begin{aligned}\frac{b^2}{c^2}\,\tfrac{3}{2}MB-\tfrac{3}{2}MC&=\frac{b^2-c^2}{c^2}(\zeta^2+\xi^2)+\zeta^2-\eta^2\\ \frac{a^2}{c^2}\,\tfrac{3}{2}MA-\tfrac{3}{2}MC&=\frac{a^2-c^2}{c^2}(\eta^2+\zeta^2)+\zeta^2-\xi^2\end{aligned}\qquad(21)$$

or,

$$\begin{aligned}\frac{b^2}{b^2-c^2}\,\tfrac{3}{2}MB-\tfrac{3}{2}\,\frac{c^2}{b^2-c^2}\,MC&=\zeta^2+\xi^2+\frac{c^2}{b^2-c^2}(\zeta^2-\eta^2)\\ \frac{a^2}{a^2-c^2}\,\tfrac{3}{2}MA-\tfrac{3}{2}\,\frac{c^2}{a^2-c^2}\,MC&=\eta^2+\zeta^2+\frac{c^2}{a^2-c^2}(\zeta^2-\xi^2)\end{aligned}\qquad(22)$$

Introducing now the values of A, B, C, these become

$$\begin{aligned}\tfrac{3}{2}M\int_0^\infty\frac{t\,dt}{D(b^2+t)\,(c^2+t)}&=\zeta^2+\xi^2\,\frac{c^2}{b^2-c^2}(\zeta^2-\eta^2)\\ \tfrac{3}{2}M\int_0^\infty\frac{t\,dt}{D(a^2+t)\,(c^2\div t)}&=\zeta^2+\eta^2\,\frac{c^2}{a^2-c^2}(\zeta^2-\xi^2)\end{aligned}\qquad(23)$$

Making $\xi=\eta=0$ in these equations and eliminating ζ we are again conducted to equation (8). Since t lies between 0 and $+\infty$ and it is understood that D denotes the positive root of

$$\sqrt{(a^2+t)\,(b^2+t)\,(c^2+t)}$$

it is clear that there can be no negative elements in either of these integrals, and they are, therefore, in all cases positive quantities. The quantities on the right hand side of equations (23) must therefore be essentially positive. As the semi-axes a, b, c are taken in order of magnitude the fractions

$$\frac{c^2}{b^2-c^2},\qquad\frac{c^2}{a^2-c^2}$$

are positive and consequently in order that the right hand members of (23) shall be positive it is necessary either that

$$\begin{aligned}\zeta&>\xi\\ \zeta&>\eta\end{aligned}\qquad(24)$$

or that,

$$\begin{aligned}\zeta^2 + \xi^2 &> \alpha^2(\zeta^2 - \eta^2)\\ \zeta^2 + \eta^2 &> \beta^2(\zeta^2 - \xi^2)\end{aligned}\tag{25}$$

where for brevity α^2, β^2 are written in place of

$$\frac{c^2}{b^2 - c^2} \quad \text{and} \quad \frac{c^2}{a^2 - c^2}$$

Write also

$$\begin{aligned}A &= \tfrac{3}{2}M\int_0^\infty \frac{tdt}{D(b^2+t)(c^2+t)},\\ B &= \tfrac{3}{2}M\int_0^\infty \frac{tdt}{D(a^2+t)(c^2+t)},\end{aligned}\tag{26}$$

Before going further, we will transform these integrals in such a way that A and B shall be given in terms of certain elliptic functions. Let k and k' denote two complementary moduli; then writing

$$k = \frac{a^2 - b}{a^2 - c^2}\tag{27}$$

we have also

$$k' = \frac{b^2 - c^2}{a^2 - c^2}\tag{28}$$

denote by x the amplitude of an elliptic integral

$$\theta = \int \frac{dX}{(\Delta k, X)}$$

also write

$$t = c^2\,\frac{\dfrac{a^2 - c^2}{c^2} - x^2}{a^2}. \qquad \left(\begin{matrix} x = 0 & \text{for } t = \infty \\ x = \infty & \text{for } t = 0\end{matrix}\right)$$

We have now

$$\begin{aligned}a^2 + t &= (a^2 - c^2)\,\frac{x^2 + 1}{x^2}\\ b^2 + t &= \frac{(b^2 - c^2)\,x^2 + (a^2 - c^2)}{x^2}\\ c^2 + t &= \frac{a^2 - c^2}{x^2}\\ dt &= -\,\frac{2(a^2 - c^2)}{x^3}\,dx\end{aligned}\tag{30}$$

Substitution of these in the expressions for A and B gives

$$A = G\int_0^\infty \frac{x^2\,dx}{(1+k'^2x^2)^{\frac{3}{2}}(x^2+1)^{\frac{1}{2}}} - H\int_0^\infty \frac{x^2\,dx}{(1+k'^2x^2)^{\frac{3}{2}}(x^2+1)^{\frac{1}{2}}}, \qquad (31)$$

$$B = G\int_0^\infty \frac{x^2\,dx}{(x+1)^{\frac{3}{2}}(1+k'^2x^2)^{\frac{1}{2}}} - H\int_0^\infty \frac{x^2\,dx}{(1+x^2)^{\frac{3}{2}}(1+k'^2x^2)^{\frac{1}{2}}},$$

where

$$G = \tfrac{3}{2}M\frac{2}{(a^2-c^2)^{\frac{3}{2}}},$$
$$H = \tfrac{3}{2}M\frac{2c^2}{(a^2-c^2)^{\frac{5}{2}}}. \qquad (32)$$

Transforming again by means of the relation

$$x = \tan. X$$

we have

$$A = G\int_0^{\frac{\pi}{2}}\left(\frac{1}{\Delta^2(k,X)} - 1\right)\frac{dX}{k^2\Delta(k,X,)} - H\int_0^{\frac{\pi}{2}}\frac{\sin.^2 X\,d\,X}{\cos.^2 X\,\Delta^3(k,X)}$$
$$(33)$$
$$B = G\int_0^{\frac{\pi}{2}}\frac{\sin.^2 X\,d\,X}{\Delta(k,X)} - H\int_0^{\frac{\pi}{2}}\frac{\sin.^2 X\,d\,X}{\cos.^2 X\,\Delta(k,X)}$$

and finally,

$$A = G\int_0^{\frac{\pi}{2}}\left(\frac{1}{dn^2\,\theta} - \right)d\theta - H\int_0^{\frac{\pi}{2}}\frac{en^2\,\theta}{dn^2\,\theta},\ \frac{su^2\,\theta}{en^2\,\theta}.\,d\theta, \qquad (34)$$

$$B = G\int_0^{\frac{\pi}{2}} sn^2\,\theta\,d\theta \qquad - H\int_0^{\frac{\pi}{2}} en^2\,\theta.\ \frac{sn^2\,\theta}{eu^2\,\theta}.$$

Still further transformation would enable us to obtain values for A and B depending upon the Θ-function, but it is not worth while to continue the process any further.

Resume now equations (23); these we see may be written in the form

$$\xi^2 - \alpha^2\,\eta^2 + [(1+\alpha^2)\,\zeta^2 - A] = 0$$
$$-\beta^2\,\xi^2 + \eta^2 + [(1+\beta^2)\,\zeta^2 - B] = 0 \qquad (35)$$

Solving for ξ^2 and η^2 gives us

$$\xi^2 = \frac{(1 + 2\alpha^2 + \alpha^2\beta^2)\,\zeta^2 - (A + \alpha^2 B)}{\alpha^2\beta^2 - 1} \qquad (36)$$

$$\eta^2 = \frac{(1 + 2\beta^2 + \alpha^2\beta^2)\,\zeta^2 - (B + \beta^2 A)}{\alpha^2\beta^2 - 1}$$

The coefficients of ζ^2 in each of these equations is always positive, and the second terms of the numerators are always positive, but the entire numerator in each case, as also the common denominator, may be either positive or negative. If given values of α and β make $\alpha^2\beta^2 < 1$ the denominator will be negative, and it will then be necessary to choose for ζ a value which will make the numerator of each of these fractions also negative. If $\alpha^2\beta^2 > 1$ the common denominator of the fractions is positive, and ζ must be so chosen that the numerators shall also be positive. Each of these determinations of ζ give positive values for ξ^2 and η^2. It is clear now that the quantities α, β, ζ cannot be chosen arbitrarily, but that if the first two of these are given the third must be determined so as to satisfy the conditions of making ξ^2 and η^2 positive. It may be noticed here that if

$$\alpha^2\beta^2 < 1$$

equations (36) give positive values of ξ^2 and η^2 by assuming $\zeta = 0$, or, the ellipsoid of three unequal axes is a possible figure of equilibrium in the case when the fluid rotates about the two principal axes, provided

$$\frac{c^2}{(b^2 - c^2)\,(a^2 - c^2)} < 1$$

or, by simple reductions

$$a^2 + b^2 - \frac{a^2 b^2}{c^2} < 0$$

We can make either ξ or η equal zero, and equilibrium will be possible by properly determining α and β.

Take now the case when $\alpha^2\beta^2 - 1 < 0$, and determine the values of ζ^2 (other than zero), which will make ξ^2 and η^2 positive. Write the first of equations (36) in the form

$$\xi^2 = \frac{\zeta^2 - \dfrac{A + \alpha^2 B}{1 + 2\alpha^2 + \alpha^2\beta^2}}{\dfrac{\alpha^2\beta^2 - 1}{1 + 2\alpha^2 + \alpha^2\beta^2}};$$

As the denominator in this is negative the numerator must also be

negative, and, therefore, for the determination of ξ^2 positive; when $\alpha^2\beta^2-1<0$ we have

$$\zeta^2 < \frac{A+\alpha^2 B}{1+2\,\alpha^2+\alpha^2\beta^2}$$

Similarly, for η^2 positive we must have

$$\zeta^2 < \frac{B+\beta^2 A}{1+2\,\beta^2+\alpha^2\beta^2}$$

For both ξ^2 and η^2 positive we have to determine ζ in such a way that both of these inequalities shall be satisfied. Now assume

$$\alpha^2\,\beta^2 - 1 < 0$$

The numerators in equations (36) must now be positive, and we must have

$$\zeta^2 < \frac{A+\alpha^2 B}{1+2\,\alpha^2+\alpha^2\beta^2} \text{ for } \xi^2 \text{ positive,}$$

$$\zeta^2 < \frac{B+\beta^2 A}{1+2\,\beta^2+\alpha^2\beta^2} \text{ for } \eta^2 \text{ positive.}$$

Finally, assume

$$\alpha^2\,\beta^2 - 1 = 0\,;$$

this obviously requires that

$$\zeta^2 = \frac{A+\alpha^2 B}{1+2\,\alpha^2+\alpha^2\beta^2} \text{ for } \xi^2 \text{ positive,}$$

$$\zeta^2 = \frac{B+\beta^2 A}{1+2\,\beta^2+\alpha^2\beta^2} \text{ for } \eta^2 \text{ positive.}$$

It is clear from these last two equations that *any* values of α and β which satisfy $\alpha^2\,\beta^2 - 1 = 0$ will not answer, but only such as shall make

$$\frac{A+\alpha^2 B}{1+2\,\alpha^2+\alpha^2\beta^2} = \frac{B+\beta^2 A}{1+2\,\beta^2+\alpha^2\beta^2}$$

Revert for a moment to equations (5), which correspond to rotation round the axis c. Combine the first and third of these and also the second and third, in order to eliminate k. We have

$$\left.\begin{aligned} \zeta_1^2\,\frac{a_1^2}{a_1^a-c_1^2} &= \frac{2}{3}\,M\int_0^\infty \frac{t\,dt}{D(a_1^2+t)(c_1^2+t)} \\ \zeta_1^2\,\frac{b_1^2}{b_1^2-c_1^2} &= \frac{3}{2}\,M\int_0^\infty \frac{t\,dt}{D(b_1^2+t)(c_1^2+t)} \end{aligned}\right\} \quad (37)$$

or

$$\zeta^2 (1+\beta_1^2) = \frac{3}{2} M \int_0^\infty \frac{t\,d\,t}{D(a_1^2+t)(c_1^2+t)}$$

$$\zeta^2 (1+\alpha_1^2) = \frac{3}{2} M \int_0^\infty \frac{t\,d\,t}{D(b_1^2+t)(c_1^2+t)} \qquad (38)$$

Where ζ_1, α_1, β_1 are written instead of ζ, α, β to denote that the rotation is around only one axis. Comparing these with (23), where α_1, β_1 may also be written instead of α, β, gives

$$(\zeta_1^2 - \zeta^2)(1+\alpha^2) = \xi^2 - \alpha^2 \eta^2$$
$$(\zeta_1^2 - \zeta^2)(1+\beta^2) = \eta^2 - \beta^2 \xi^2$$

We can clearly determine ξ, η, ζ in such a way that these equations shall be satisfied. The interpretation of this is simply that certain ellipsoids, which are figures of equilibrium when the rotation is around one principal axis, will also be figures of equilibrium when the rotation is around all three of the principal axes. The consideration of the case when the ellipsoids are of rotation need not be entered into, as the conditions for this case follow very simply from the formulæ already given for the more general case.

There is another way of attacking the general problem of the rotation of a fluid ellipsoid, which is much more general in its nature than the preceding. If we denote by u, v, w the component velocities of a particle of the fluid mass, whose position at any time is x, y, z, then these quantities are connected with the ξ, η, ζ of the preceding pages by the relatives

$$\begin{aligned} u &= z\,\eta - y\,\zeta \\ v &= x\,\zeta - z\,\xi \\ w &= y\,\xi - x\,\eta \end{aligned} \qquad (39)$$

and, as is well known from the principles of theoretical mechanics, consequently ξ, η, ζ denote the velocities which the ellipsoid would have if rotating as a rigid body. If the fluid, however, have a motion relatively to the ellipsoid, these quantities ξ, η, ζ are increased by certain other components, which call

$$\Xi,\ H,\ Z$$

Then, if the new components of the rotation of the ellipsoid about its axes are denoted by p, q, r, we have

$$\begin{aligned} p &= \xi + \Xi, \\ q &= \eta + H, \\ r &= \zeta + Z. \end{aligned} \tag{40}$$

And writing α, β, γ the component relative velocities of the particle

$$\begin{aligned} \alpha &= u + yr - zq, \\ \beta &= v + zp - xr, \\ \gamma &= w + xq - yp. \end{aligned} \tag{41}$$

By the introduction of the new velocities Ξ, H, Z we have now to write for u, v, w the expressions

$$\begin{aligned} u &= z\left(\eta + \frac{c^2 - a^2}{c^2 + a^2} H\right) + y\left(-\zeta + \frac{a^2 - b^2}{a^2 + b^2} Z\right) \\ v &= x\left(\zeta + \frac{a^2 - b^2}{a^2 + b^2} Z\right) + z\left(-\xi + \frac{b^2 - c^2}{b^2 + c^2} \Xi\right) \\ w &= y\left(\xi + \frac{b^2 - c^2}{b^2 + c^2} \Xi\right) + x\left(-\eta + \frac{c^2 - a^2}{c^2 + a^2} H\right) \end{aligned} \tag{42}$$

and consequently by substitution in (41)

$$\begin{aligned} \alpha &= 2a^2 \left\{ \frac{Zy}{a^2 + b^2} - \frac{Hz}{c^2 + a^2} \right\} \\ \beta &= 2b^2 \left\{ \frac{\Xi z}{b^2 + c^2} - \frac{Zx}{a^2 + b^2} \right\} \\ \gamma &= 2c^2 \left\{ \frac{Hx}{c^2 + a^2} - \frac{\Xi y}{b^2 + c^2} \right\} \end{aligned} \tag{43}$$

Multiplying these by x, y, z, respectively, and dividing by a^2, b^2, c^2, we have, on addition,

$$\frac{\alpha x}{a^2} + \frac{\beta y}{b^2} + \frac{\gamma z}{c^2} = 0 \tag{44}$$

and this expresses the fact that a particle which at any time lies on a given ellipsoid will throughout the motion lie either on this ellipsoid or on one similar to it. This is similar to one of the properties of the motion of a fluid particle in case a solid ellipsoid rotates in an infinite mass of fluid, viz. (*American Journal of Mathematics*, vol. 2, page 271), that the co-ordinates of a fluid particle are always expressible as functions of the parameter of the ellipsoid upon which it lies, which is confocal to the given ellipsoid. Our problem being now one in ordinary hydrodynamics, we can employ the equations of hydrodynamics for its further elucidation. It will not be necessary here to enter into a discussion of how the equations of motion are obtained in

the case of moving axes, and I shall merely write the equations at once. They may be found in Riemann's celebrated paper, "*über die Bewegung eines flüssigen gleichartigen Ellipsoides*," though not in the form given here. Denoting by p the fluid pressure, by ρ the constant density, and by V the potential of applied forces, we have

$$\frac{1}{\rho}\frac{dp}{dx}+\frac{dV}{dx}+w\,q-v\,r+\alpha\frac{du}{dx}+\beta\frac{du}{dy}+\gamma\frac{du}{dz}+\frac{du}{dt}=0$$

$$\frac{1}{\rho}\frac{dp}{dy}+\frac{dV}{dy}+u\,r-w\,p+\alpha\frac{dv}{dx}+\beta\frac{dv}{dy}+\gamma\frac{dv}{dz}+\frac{dv}{dt}=0 \quad (45)$$

$$\frac{1}{\rho}\frac{dp}{dz}+\frac{dV}{dz}+v\,p-u\,q+\alpha\frac{dw}{dx}+\beta\frac{dw}{dy}+\gamma\frac{dw}{dz}+\frac{dw}{dt}=0$$

If, as in Riemann's case, we assume that the only forces acting are those due to the mutual attraction of the particles of the fluid ellipsoid, we have

$$\frac{dV}{dx}=A\,x,\text{ etc.,} \quad (46)$$

where

$$A=\frac{3}{2}M\int_0^\infty\frac{d\lambda}{(a^2+\lambda)N},$$

$$B=\frac{3}{2}M\int_0^\infty\frac{d\lambda}{(b^2+\lambda)N}, \quad (47)$$

$$C=\frac{3}{2}M\int_0^\infty\frac{d\lambda}{(c^2+\lambda)N},$$

and

$$N=\sqrt{(a^2+\lambda)\,(b^2+\lambda)\,(c^2+\lambda)}$$

The values of these quantities are given in the article above referred to in the *American Journal of Mathematics*. Before going further observe that, no external forces acting, if we denote by $\omega_1\ \omega_2\ \omega_3$ the components of angular momentum, we have from mechanics

$$\frac{d\omega_1}{dt}-\omega_2\,r+\omega_3\,q=0$$

$$\frac{d\omega_2}{dt}-\omega_3\,p+\omega_1\,r=0, \quad (48)$$

$$\frac{d\omega_3}{dt}-\omega_1\,q+\omega_2\,p=0;$$

and since

$$\begin{aligned} \omega_1 &= \Sigma m\,(wy - vz), \\ \omega_2 &= \Sigma m\,(uz - wx), \\ \omega_3 &= \Sigma m\,(vx - uy), \end{aligned} \tag{49}$$

we can readily find

$$\begin{aligned} \omega_1 &= \frac{M}{5}\left\{ \frac{(b^2-c^2)^2}{b^2+c^2}\,\Xi + (b^2+c^2)\,\xi \right\} \\ \omega_2 &= \frac{M}{5}\left\{ \frac{(c^2-a^2)^2}{c^2+a^2}\,H + (c^2+a^2)\,\eta \right\} \\ \omega_3 &= \frac{M}{5}\left\{ \frac{(a^2-b^2)^2}{a^2+b^2}\,Z + (a^2+b^2)\,\zeta \right\} \end{aligned} \tag{50}$$

and (47) now become

$$\left\{ \frac{(b^2-c^2)^2}{b^2+c^2}\,\frac{d\Xi}{dt} + (b^2+c^2)\,\frac{d\xi}{dt} \right\} + \left\{ \frac{(a^2-b^2)^2}{a^2+b^2}\,Z + (a^2+b^2)\,\zeta \right\} q$$
$$- \left\{ \frac{(c^2-a^2)^2}{c^2+a^2}\,H + (c^2+a^2)\,\eta \right\} r = 0, \text{ etc.} \tag{51}$$

The other two equations being easily written down by merely advancing the letters.

Eliminating the pressure from equations (45), in the usual manner, and substituting for u, v, w their values, we have

$$\begin{aligned} \frac{1}{a^2}\,\frac{d\xi}{dt} &= 2\left\{ \frac{Z\,\eta}{a^2+b^2} - \frac{H\,\zeta}{a^2+c^2} \right\}, \\ \frac{1}{b^2}\,\frac{d\eta}{dt} &= 2\left\{ \frac{\Xi\,\zeta}{b^2+c^2} + \frac{Z\,\xi}{a^2+b^2} \right\}, \\ \frac{1}{c^2}\,\frac{d\eta}{dt} &= 2\left\{ \frac{H\,\xi}{c^2+a^2} - \frac{\Xi\,\eta}{b^2+c^2} \right\}; \end{aligned} \tag{52}$$

multiplying these by ξ, η, ζ, respectively, adding and integrating, we have

$$\frac{\xi^2}{C_1\,a^2} + \frac{\eta^2}{C_1\,b^2} + \frac{\zeta^2}{C_1\,c^2} = 1 \tag{53}$$

where C_1 is a constant. Substituting in (45) the values of the different quantities therein contained, we have, on performing some complicated reductions:

$$\frac{1}{\rho}\,\frac{dp}{dx} + \left\{ A + \frac{4\,c^2\,(c^2-a^2)}{(c^2+a^2)^2}\,H^2 - \left(\frac{c^2-a^2}{c^2+a^2} = \eta\right)^2 \right.$$
$$\left. - \frac{4\,b^2\,(a^2-b^2)}{(a^2+b^2)^2}\,Z^2 - \left(\frac{a^2-b^2}{a^2+b^2} + \zeta\right)^2 \right\} x = 0, \text{ etc.} \tag{54}$$

The other equations are readily written down. The coefficients of y and z in the first of (54) vanish by virtue of (51). These may all be written briefly, as

$$\begin{aligned} \frac{1}{\rho}\frac{dp}{dx} + L\,x &= 0 \\ \frac{1}{\rho}\frac{dp}{dy} + M\,y &= 0 \\ \frac{1}{\rho}\frac{dp}{dz} + N\,z &= 0 \end{aligned} \qquad (55)$$

from which

$$\frac{p}{\rho} + \tfrac{1}{2}(Lx^2 + My^2 + Nz^2) = \text{const.} \qquad (56)$$

The surfaces of equal pressure are thus

$$Lx^2 + My^2 + Nz^2 = \text{const.}$$

It does not seem possible in this case to have constant values for L, M, N which shall be proportional to

$$\frac{1}{a^2}, \frac{1}{b^2}, \frac{1}{c^2}.$$

A further investigation would make it necessary to introduce functions of a higher order than elliptic, so I shall not, at present, attempt to go further in this interesting and rather curious problem.

It was my intention to add here a note on the motion of the fluid mass when the rotation is around the mean axis of the ellipsoid, but after having written out a portion of the work I discovered that that particular case has been fully worked out, in a most elegant manner, by Mr. A. G. Greenhill, in Vol. III of the Proceedings of the Cambridge Philosophical Society; I take pleasure, therefore, in referring the reader to that source for further information, which is there given in a much better manner than I could possibly present it. I trust that Mr. Greenhill may see fit at some future time to take up the more general problem of which I have here given a brief account.

Washington, D. C., Sept. 28, 1880.

A NEWLY DISCOVERED PROPERTY OF THE ELLIPSE, AND ITS APPLICATION TO THE "OVAL CHUCK."

By Frank M. Leavitt.

In studying to devise a machine for a certain purpose, the writer stumbled upon a mathematical property of the ellipse which he thinks to be not devoid of interest, on account of its apparently not having been noticed in any published treatment of the ellipse, and also on account of its practical utility for the purpose below mentioned.

In turning an ellipse on a lathe provided with an "oval chuck" the disadvantage is met with that at only four points, viz., at the extremities of the major and minor axes, is the surface of the work in a position normal to the cutting tool, half of the time the upper face of the latter being at an acute angle with the tangent to the ellipse at the point of cutting, whereby the tendency is to spring the tool away from its work, and during the remainder of the time, the angle formed being obtuse, the pressure tends to spring the tool inward, with the liability of making too deep a cut.

A similar state of things exists when a blank of sheet metal is being trimmed in an "oval chuck," and it is in this case so serious an obstacle as to make it totally impossible to obtain a satisfactory result.

It was while studying for a remedy in this latter case that the following property was noticed:

If an indefinite line be drawn through the centre of an ellipse, and if through a point on this line situated at a constant distance from the point of intersection with the ellipse (and outside the latter) a line be drawn making with the transverse axis of the ellipse an angle equal to the angle made with the same by the line drawn through the centre, the portion of the prolongation of the line so drawn included between its point of intersection with the line through the centre of the ellipse and a normal to the latter at its intersected point, will be of a constant length.

To make this clear by Fig. 1, draw any line through the centre, as $O\ D$. At the constant distance $A\ D = C$ draw through the point D a line $M\ D\ E$, making the angle $D\ M\ O = D\ O\ M$. From the point A draw the normal $A\ E$. Then will the distance $E\ D$, included

between the normal and the line $O\,D$ be constant for all positions, in the same ellipse, of the line $O\,D$.

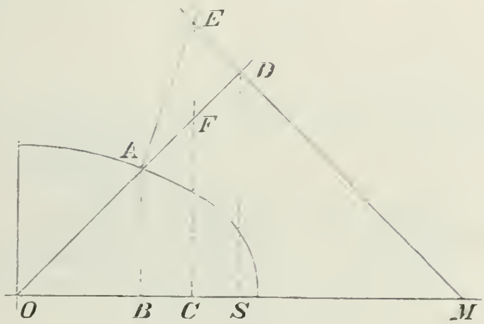


Fig. 1.

This may be proved as follows:

Let r be the *radius vector* $O\,A$ of the point of intersection A, α the angle $A\,O\,B = E\,M\,O$, x'' and y'' the co-ordinates of the point A, the origin of co-ordinates being at the centre of the ellipse, $x_{\prime}$ and $y_{\prime}$ the co-ordinates of the point E, and a and b respectively the semi-transverse and semi-conjugate diameters of the ellipse. Let $E\,D = R$

We have
$$x_{\prime} = \cos.\ \alpha\ (r + c - R)$$
$$y_{\prime} = \sin.\ \alpha\ (r - c + R)$$
also,
$$x'' = r \cos.\ \alpha$$
$$y'' = r \sin.\ \alpha$$
which values, substituted in the equation of a normal to the ellipse,
$$(y - y'')\,b^2\,x'' = (x - x'')\,a^2\,y''$$
gives after reducing
$$R = \frac{a^2 - b^2}{a^2 + b^2}\,c.$$

Hence R is constant for each ellipse, and its value depends on the values of the major and minor axes of the same, multiplied by the constant c, and varies from $R = 0$ when $a = b$, or in the case of a circle, to $R = c$ when $b = 0$.

The manner in which this principle may be applied to a lathe furnished with an "oval chuck" is illustrated by the skeleton drawing Fig. 2, which is lettered similarly to Fig. 1.

The slotted arm $A\,E$ swings freely on the pivot A, which is fixed to the carriage of the lathe, and to this arm is fastened the cutting tool t by a proper tool post not shown in the sketch. The tool must

be so placed that its cutting edge is in line with the centre of motion A, of the arm $A\,E$, and of course enough in front of the pivot on which the arm vibrates as not to allow the latter to interfere with the work.

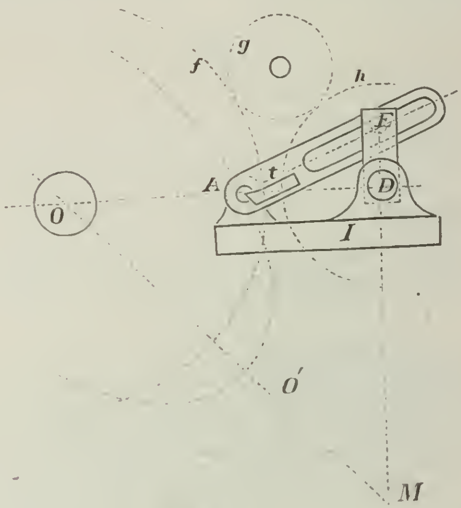


Fig. 2.

On the lathe carriage, at any convenient distance from the centre of motion A, is a rotating shaft D, which is geared in some way with the main spindle, O, of the lathe, so as to make two revolutions to the latter's one, and in the same direction. This shaft D is provided with a crank, $D\,E$, the crank-pin of which slides in the slot in the arm $A\,E$, and is so arranged as to be adjustable to any desired distance from the centre of the shaft.

When the transverse axis of the ellipse to be turned coincides with the line $O\,A\,D$ the crank $E\,D$ must be on its inner centre, that is, with its crank-pin in the line $O\,A\,D$, and between O and D. Then, since the shaft D has twice the angular velocity of the spindle O, it follows that at all times the angle $A\,D\,E$ formed by the crank with the line $O\,D$ will be twice as great as that formed by the transverse axis with the line $O\,D$, or $A\,D\,E = 2\,D\,O\,M$, a condition required in our proposition. Also, the fixed distance $A\,D$ represents the constant c, the crank $D\,E$ being the constant R. It therefore follows, from the conditions above set forth, that in all positions of the chuck the arm $A\,E$, and consequently the tool t, will be in a normal position to the periphery of the ellipse at the point of cutting, which is the condition sought for.

The mechanism may be adapted to the turning of ellipses of different eccentricity by simply adjusting the distance of the crank pin from the centre of the shaft D so that its value will be

$$R = \frac{a_2 - b_2}{a_2 + b_2} c.$$

It is evident that if a pair of rotary cutters be substituted for the turning tool, in trimming sheet metal blanks, the required result will be obtaiued in a similar manner.

A SIMPLE TRANSMISSION DYNAMOMETER.

By Elihu Thomson.

While engaged in the work of constructing and testing dynamo-electric machines of various sizes and capacities, the need was felt of a simple instrument, by means of which a measurement of power transmitted to a machine could be readily obtained. To supply this need, the dynamometer which forms the subject of this paper, was devised. It is a modification of the dynamometer which, I believe, was invented by Herr von Hefner-Altenech, and used by Dr. J. Hopkinson in his tests of Siemens' dynamo-electric machines.

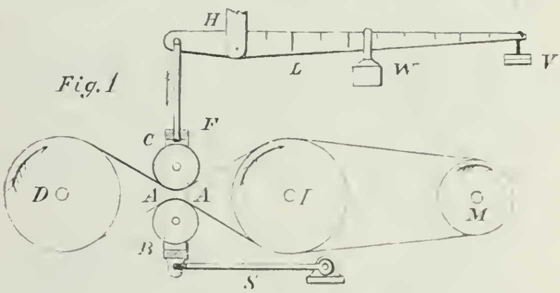


Fig. 1 represents a side elevation of the essential parts and their relations to one another. D is the driving pulley, from which power is conveyed to the driven pulley M by a belt. The pulley M, in my experiments, was upon the shaft of the dynamo-electric machine which consumed the transmitted power. The relative sizes of the pulleys D and M is immaterial. Between them, and distant from D its own diameter, is mounted an idle pulley I, of the same diameter as that of D. Between the driving pulley D and the idle pulley I are

two small idle pulleys, *B C*, mounted to run freely in a movable frame, *F*. The pulleys *B C* are placed at such a distance apart that the angles *A A*, formed by lines of the belt leaving *D* and *I*, shall be each 60°. The frame *F*, supporting the pulleys *B C*, is hung upon one extremity of a horizontal lever, *L*, whose fulcrum is at *H*. The frame *F* and pulleys *B C* are free to move in a vertical direction through a small range, being suitably guided, as at *S*. The lever *L* is provided with a sliding weight, *W*, and has divisions marked upon it equal to, or fractions of, the distance from the fulcrum *H* to the points of attachment of the frame *F* to the lever *L*.

The weight *W* is first removed, and the lever *L* and counterpoise *V* made to exactly balance the frame *F*. This adjustment is made when the parts are free to move, and once made need not be repeated. It matters not what may be the tension of the belt used, the equilibrium will remain undisturbed, because the strains are counterbalanced upon the pulleys *B* and *C*. Let now rotary motion be imparted, as shown by the arrows, and all parts be assumed to run without friction or resistance; the equilibrium of the lever and frame will not be disturbed. Further, let there be a consumption of power in turning the pulley *M*. In this case the lower side of the belt will become more tense than the upper side, and in consequence, the pulley *B* having to support this added tension, and *C* being relieved of part of the tension, there results a downward tendency of the pulley *B*, followed by *C* and the frame *F*. The lever *L* is consequently thrown out of equilibrium. By placing the weight *W* upon the lever *L*, so as to restore the equilibrium, we will be able to obtain an exact measure, in pounds, of the difference of tension of the two sides of the belt, which, multiplied by the belt speed in feet per minute, gives the power in foot-pounds per minute.

Thus, if the weight *W* be 33 lbs. and to produce equilibrium, it is required to be placed upon the lever *L* at a distance from *H* equal to that of the frame *F* on the other side, then the tendency of the frame *F* to move downwards is 33 lbs. But when the angles *A A* equal 60° each, the downward tendency of the frame *F* equals the difference of tension of the two sides of the belt, or the difference in longitudinal strains of the driving side and loose side. To determine the belt speed per minute it is of course only necessary to know the circumference in feet and number of revolutions of either of the pulleys *D*, *I* or *M*, and multiply these quantities together. In the case

assumed let the product, or belt speed, be 1000 feet, which, multiplied by the tension difference, 33 lbs., is 33,000, or one horse-power.

Fig. 2

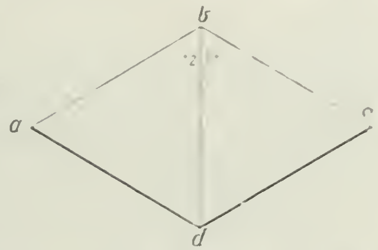


Fig. 2 is intended to elucidate the effect produced upon the frame F by the increase of the tension of the driving side of the belt, and to show that the downward tendency of said frame is a measure of the difference of longitudinal strain of the tight and loose sides of the belt. Since the strains upon the pulleys B and C, due merely to the tightness of the belt, irrespectively of its function in conveying power, are equal upon each, these strains neutralize each other, and may therefore be neglected. In Fig. 2 let $a\ b$ and $b\ c$ represent the differences of belt strain due to power transmission, on the portions of belt upon each side of the frame F; that is, $a\ b$ represents the difference of strain of two portions of the belt, one passing tangentially from the pulley D to the pulley C, and the other portion passing tangentially from D to B. Similarly, $b\ c$ represents the difference of strain of those portions passing tangentially from the pulley I to B and C respectively. These differences of strain act upon the pulley B at an angle of 120° to each other, as will be seen from the direction taken by the belt. The resultant, $b\ d$, Fig. 2, is equal to $a\ b$ or $b\ c$ and is downward. This resultant is counterbalanced by the sliding weight W and measured upon the lever L used as a steelyard.

When properly constructed, so that the parts have sufficient freedom of motion, this dynamometer is capable of showing quite small variations of power consumed, and it has the advantage that the measurements are taken directly from the driving belt and not from intervening apparatus involving considerable friction. When the upper side of the belt is the tight or driving side, suitable modifications can be made in the arrangement of parts, and will readily suggest themselves. The dynamometer which I have used has about the following dimensions: Pulley $D = 32$ in. diam.; $I = 32$ in. diam.; distance between D and $I = 32$ in.; diameter of pulleys B and $C = 8$ in.;

distance from centre to centre of B and $C = 10$ in.; length of the lever $L = 80$ in.; the support, or fulcrum, H is 10 in. from the point of attachment of the frame F, and the lever L is divided on the long arm into divisions of 5 inches, marked 1, 2, 3, etc.; the weight W is 33 lbs., and belt speed 2000 feet per minute.

In this case the numbers 1, 2, 3, etc., on the lever L read horse-powers. The belt speed, of course, varies within certain limits, and allowance is made for its variations.

METHODS FOR JUDGING OF THE WHOLESOMENESS OF DRINKING WATER.*

By Reuben Haines.

Many years ago the usual way to ascertain the wholesomeness of drinking water was to discover what mineral impurities were present. The water was therefore evaporated to dryness, and a more or less complete analysis was made of the solid mineral substance left, as is done with mineral-spring waters. Little attention was given to the organic matter, except when present in very large amounts, as in marsh water, etc. Occasionally, even now, we read of some one making tests for mineral impurities in a suspected well water, but omitting any test for the organic matter. Such analyses were exceedingly troublesome; and, after all, it is very doubtful whether they were of much value—in many cases perhaps of no value whatever, in ascertaining the wholesomeness of ordinary drinking water. Of course, the detection of iron and of sulphate of lime and magnesia, in this way, was of some importance, for waters containing considerable amounts of these substances should be considered unwholesome for daily use, in health. But we really have no reason at all for supposing that such mineral substances as silica, alumina, potash and soda, as they occur in any, but very rare cases, have any influence whatever on health. Moreover, waters which are free from much mineral salts may often be very unwholesome for other reasons not discoverable at all in this way. This sort of analysis has therefore been entirely discarded by chemists conversant with recent sanitary experience, except in selecting a new source of water

* Abstracts of lectures delivered before the Franklin Institute, December, 1880, in which additional original matter has been introduced.

supply for a city, which involves other than sanitary interests as well.

In the preceding lecture it was shown that by far the most important element in sanitary investigations of water, is the organic matter contained in it, either in the form of minute suspended particles or in actual solution. It was shown that the purest rain and spring waters contain a minute amount of organic matter, but that rivers, streams, and shallow wells in populous districts, contain much larger proportions of it. It has long been known that waters, the sources of which originate in populous districts, were often the apparent cause of disease.

These facts have been for many years recognized by chemists, and it has therefore been their endeavor to devise methods for finding the exact amount and also the nature of this organic material. Let us now briefly review the advances which have been made in this direction.

The earliest method used for this purpose was what is known as the "ignition" process. It consisted in evaporating a measured quantity of water to dryness at a temperature which varied with different chemists, and weighing the residue. This residue was then exposed in a platinum dish to a sufficient heat to burn away all the organic matter. On cooling, the weight was again taken, and the difference was called organic matter. It was very simple, and easily performed, and was extensively practiced. It has been shown, however, to give very erroneous results in a large number of cases, especially with hard waters, and well waters containing considerable nitrate. There are numerous chemical objections to it which render it entirely fallacious, but it will not be proper on the present occasion to enter into much chemical detail. We may say, however, that any carbonates of lime, etc., will, by the heat necessary to burn away the organic matter, partially lose their carbonic acid, ammoniacal salts will be volatilized, nitrates will be converted into carbonates by the carbon of the organic matter, these and other salts will lose water of crystallization, and if much chloride of sodium is present, in contact with carbon, hydrochloric acid will be volatilized, the fumes of which are often perceptible in thus treating the residue of a highly polluted water. Moreover, it is frequently difficult to get rid of the last traces of unconsumed carbon without raising the intensity of the heat so as to render the loss of mineral salts positively certain. In fact, we can never know what interchanges may take place among the materials of the residue when heated to faint redness. Various efforts were made to avoid this, or to replace the loss. Carbonate of soda was added during the first evaporation, but it has

been shown that by this means a part of the organic matter was liable to be destroyed before the first weight was taken. Carbonate of ammonia, or carbonic acid water was added to the residue after being heated, so as to restore the lost carbonic acid, but while this confessedly replaces only a part of the loss, it has also been shown that the weight of the residue is frequently increased in a curious and irregular manner by this means, so as finally to become sometimes considerably greater than it was before being heated at all.

Prof. Wm. Ripley Nichols finds this method, with the employment of aqueous carbonic acid, of some utility for the relative comparison of the very soft waters of many New England streams. In these cases the loss appears to represent chiefly organic matter, and is recorded as "organic and volatile matter," which is the only accurate designation for it. But the results probably can never be, constantly, very exact, and no reliance should be placed upon them to the exclusion of other tests. In the case of the majority of well waters Prof. Nichols considers this method valueless. It may frequently be useful, and as a qualitative test only, to heat the solid residue, and notice the odor, if any is given off. This will aid us in distinguishing animal from vegetable substances.

The unsatisfactory character of the "ignition" method led chemists to adopt what is known as the "permanganate" method. Of this there are numerous modifications, giving different results according to the practice of different chemists. They are all based on the same general principle, which is strikingly illustrated by the action of potassic permanganate on oxalic acid in solution. The magnificent color of the permanganate almost instantly disappears in a surprising manner. If we pour very rapidly, numerous bubbles of gas rise and burst at the surface, exactly as in a glass of soda water. Now, what happens here is, chemically, just as truly combustion, as the burning of coal in a fire, only the oxidation takes place in water instead of air. The permanganate is rich in oxygen, which it gives up very readily, and this combines with the oxalic acid to form carbonic acid and water, just as the oxygen of the air unites with the burning coal to form the same substances. The oxalic acid is destroyed and the resulting carbonic acid gas passes off in small bubbles. The permanganate also is destroyed, as is shown by the disappearance of color, and the hydrated oxide of manganese, which would otherwise settle in brown particles, is held in

solution by sulphuric acid, a little of which was previously added: forming the colorless manganese sulphate.

Now oxalic acid is only one of the many organic substances upon which potassic permanganate acts in the same way, in a greater or less degree. As long as there is organic substance in the liquid to be acted upon, the color of the permanganate will be destroyed, although, in some cases, very slowly; but as soon as this organic substance is exhausted the pink color will remain permanent in the liquid. We obtain in this way a sort of measure of the amount of organic substance. In the actual analysis the chemist takes a measured quantity of the sample of water and pours into it from a measuring-tube, called a burette, a solution of permanganate of known strength. This is a very imperfect description of the method, but it is sufficient for our purpose.

This method, with whatever modification it is used, does not really show how much organic matter is actually present, but only how much oxygen has been required by the substances which are capable of being oxidized in this way. German chemists are in the habit of reckoning five times the numbers obtained in the analysis as representing the total amount of "Organischer Substanz." This is, however, an arbitrary factor, and while it may have been tolerably accurate for some waters used as test-analyses, we have no proof at all that the calculation would be accurate in all other cases, and it is hence reduced to a mere guess.

There are many objections to this method which show it to be very unreliable. Different substances are acted upon in very different degrees by permanganate, and upon some that must frequently occur in polluted water there is no action at all. Hence the method cannot by any means be relied upon to give the absolute amount of organic substance. If nitrous acid, ammonia, hydrogen sulphide, or protoxide of iron are present in the water they will affect the permanganate in the same way as organic matter, and would be counted as such in the analysis. We must, therefore, find the amount of these substances in other ways and make a correction for them. It is also stated to be uncertain whether the action on ammonia is uniform, and if it is not, no accurate correction for the ammonia can be made. Dr. Frankland, while condemning this method as entirely unreliable for ascertaining the *quantity* of organic matter, admits that it may be a useful *qualitative* test. "Thus," he says, "if a clear and colorless water decolorizes much of the perman-

ganate solution the water ought to be rejected for domestic use, as being of doubtful quality." It has the advantage of being readily performed, as a qualitative test, in a short time, with few materials and no special apparatus, and is therefore useful on occasions when there is no opportunity for a regular analysis.

In the hands of any one chemist, its use as a quantitative method will, without doubt, give some valuable information as to the relative quality of different waters, if the test is always performed in the same manner. The degree of rapidity with which the oxidation takes place will probably give some idea as to the putrescible nature of the organic matter. It is thus that Dr. Tidy's periodical analyses of the metropolitan water of London, by the permanganate method, have not been without value.

In England and America both the methods I have described have been to a great extent abandoned, and two other methods have taken their place. These are Wanklyn's ammonia method, and Frankland's combustion method, which were devised and published at nearly the same time in 1867 and 1868. The Frankland process consists in evaporating a measured quantity of water to dryness and burning the residue in a combustion-tube on the same principle as that by which an organic analysis is made. The resulting gases instead of being immediately absorbed are measured in a delicate and complicated apparatus for gas analysis, and then separated by absorption. The gases resulting from the organic matter are chiefly carbonic acid and nitrogen, and are calculated as organic carbon and organic nitrogen. The gases are drawn out of the combustion-tube by means of a Sprengel vacuum pump, and upon the perfection of the vacuum produced, depends, in part, the reliability of the results.

The Wanklyn method, which is much more generally practiced, makes use of the potassic permanganate. A measured quantity of the water to be analyzed is put into a glass retort, to which is attached a condenser. A little carbonate of soda is dropped in through the stoppered orifice in the retort, and the water is distilled rapidly until no more ammonia is given off, and condensed with the steam. The distilled water is collected in flat-bottomed test-tubes of perfectly colorless glass. This ammonia, which is first distilled, is called free ammonia. It is that which is present, in the form of ammonia, either in the free state or in combination, in the water. A solution of potassic permanganate, with which a strong solution of caustic potash has been

mixed, is now poured into the retort, and the distillation continued until no more ammonia is perceived in the distilled water. The ammonia now found in the distillate is called "albuminoid ammonia," and is that which results from the decomposition of nitrogenous organic matter by the action of the permanganate solution. Hence it represents that organic matter, and forms a relative, more or less definite, measure of it. It is upon this peculiar action of a boiling, strongly alkaline solution of potassic permanganate that the method of Wanklyn is founded. The delicacy of the method depends upon the sensitiveness of the Nessler test for ammonia, which is one of the most delicate colorometric tests in the whole range of analytical chemistry, being capable of recognizing easily one part of ammonia in ten million parts of water, and distinguishing differences of one-third of this amount.

Let us now consider some of the defects of these two methods of analysis and appreciate what may be really learned by their use.

In the first place, by neither of them can we, with certainty, estimate the exact amount of organic matter actually present in water. For this purpose there is no method known; nor can these methods enable us to identify and separate the different kinds of organic substances that may be present. In fact, we know almost nothing as to their nature, except that there is a general impression that much of this polluting material is probably of an albuminoid character.

Frankland's method endeavors to estimate the exact quantity of organic matter, but, as will be seen by a candid examination of the essential defects of the process, there is at least a formidable array of probabilities against the possibility of its doing this.

Wanklyn, on the other hand, does not undertake to estimate the absolute quantity, but simply attempts to find a factor by which to make a tolerably accurate comparison of the relative purity or impurity of water. By this method we analyze a naturally good water, known beforehand to be wholesome by long experience and absence of any contaminating source, and the results obtained from this are then taken as a *standard* by which to compare other waters. The opinion is expressed by some chemists that this standard, although practically constant for any one locality, will be apt to vary considerably for different localities, even remote from the seaside.

In comparing the two methods we may say that both are philosophical in some points, and both unphilosophical in other ways.

That of Frankland seems theoretically better, because a combustion is made of the organic matter on precisely the same principle that is employed in the ultimate analysis of an organic compound. By this means the carbonaceous matter not nitrogenous is also estimated, which does not enter into the results of Wanklyn's method.

But Wanklyn's method is more philosophical, because it deals with the water itself, and not with merely the solid residue left after evaporating the water. We want to know what is contained in the water, or at least the properties of the water; but we do not necessarily need to know what is in the residue, for it is at best only indirect evidence, and what is shown by it may be only partially true of the water from which the residue came.

The value of Frankland's process depends on the assumption that what is contained in the residue fairly represents what is contained in the water itself, deducting of course the nitrates, which must be got rid of in conducting this method. This is really an assumption which Frankland has never clearly proved to be a fact, and which Wanklyn and others, including German authorities, claim is a mistake. It has been shown by German chemists that an appreciable amount of organic matter is lost during the evaporation of the water, especially when originally volatile matter is present. Moreover, we have no knowledge as to whether volatile products may not be formed during the evaporation at the boiling temperature. Frankland endeavors to prevent the latter result by the addition of sulphurous acid, taking advantage of its antiseptic properties. This has, however, been shown to be quite objectionable, causing the inevitable loss of part of the organic matter through the formation of gradually concentrating sulphuric acid. It is claimed that the further addition of sodium sulphite will not sufficiently neutralize this acid; but whether this is correct or not may not have been proved. Several other technical objections* have been advanced against the method, which it will be unnecessary to quote here. Finally, the chances of error, and the numerical corrections to be made are so numerous as to make this method quite complicated and difficult. It requires the chemist to be thoroughly skilled in the most delicate gas analysis, on account of the exceeding minuteness of the quantities to be measured, as compared with ordinary analysis. The apparatus is costly and fragile, and considerable time is required

* *Vide Jour. Chem. Soc.*, *Chem. News*, etc.

for each analysis. Skilful chemists might not consider some of these as really serious objections, but they will certainly prevent the general adoption of this method by City and State Boards of Health for sanitary purposes.

This method endeavors, also, to determine the character of the organic matter by the proportionate relation of the organic nitrogen, to the organic carbon, this relation being found to be, by an average between wide limits, respectively, 1 : 11·9 for waters containing extract of peat, and 1 : 1·8 for sewage.

But Frankland has found that oxidation of peaty matter decreases the carbon, while oxidation of sewage decreases the organic nitrogen. "It is thus evident that the proportions of nitrogen to carbon in soluble vegetable and animal organic matters vary in opposite directions during oxidation—a fact which renders more difficult the decision as to whether the organic matter present in any given sample of water is of vegetable or animal origin."

Prof. Nichols quotes* from Sander: "Without a knowledge of the previous history of the water, the relative proportion (between carbon and nitrogen) is not available as a means of deciding as to the nature of the contamination; if, however, the previous history of a water is known, there is scarcely need of so particular an analysis in order to judge of its character." †

It has been shown that in the case of very pure waters the experimental error may often be greater than the total amount of organic material to be estimated, and that in the case of waters containing readily decomposable nitrogenous organic matter, together with a large excess of nitrates, the accuracy of the results may be more or less vitiated by the efforts to get rid of the latter. Mr. Wigner says that "supposing that the organic nitrogen yielded by the Frankland and Armstrong process were a positive quantity instead of a quantity needing a heavy correction for personal equation and for impurities in the chemicals used, yet the danger of error involved in the analysis, and the risk of contamination by atmospheric impurities, are in my opinion sufficient to prevent it from ever coming into general use; and unless generally used it is undesirable for reports which appeal to public sense and public understanding." ‡

* Prof. Nichols' paper in Buck's *Hygiene*, vol. 1, page 303.

† "Handbuch der öffentlichen Gesundheitspflege," p. 230.

‡ *Sanitary Record*, Oct. 19, 1877.

Wanklyn's method is also unsatisfactory and unphilosophical as a scientific quantitative method, because it assumes that most of the organic material usually found in drinking water is in its general character similar to animal and vegetable albumin. This is an assumption which no one has yet proved to be correct, and it is difficult to perceive how it can be proved until we know definitely what these specific materials are, which it is impossible to determine in the present state of our knowledge. Wanklyn found that pure albumin yielded by this method about two-thirds of its total nitrogen as ammonia, and that this proportion was quite constant. That this is also true of organic matter in water cannot at present be proved. He proposed at first to calculate the total organic matter as ten times the albuminoid ammonia, but this, he has since, evidently, and it should be said rightly, rejected as both unscientific, and really not necessary for the practical judgment of the sanitary character of water by his method.

Those who are familiar with the most recent sanitary experience realize that it is the *quality* rather than the *absolute quantity* of organic matter that is the most important factor in the sanitary judgment of a drinking water. A water which contains a large amount of one kind of organic substance may be much more wholesome, or far less unwholesome, than that which contains only a small amount of another kind. It is a matter of actual experience that a water, notwithstanding it contains a large amount of nitrogenous organic matter capable of yielding albuminoid ammonia, may be found to be practically wholesome, or at least may be drunk for a long period without apparently producing any injurious effects; while, on the other hand, a water which contains *even a minimum* of organic substance capable of yielding albuminoid ammonia may nevertheless contain or develop the *materies morbi* or unknown causal "something," of a specific disease.

While Wanklyn's ammonia method is certainly of very easy and expeditious performance, yet great caution is necessary in the formation of an opinion from the analytical results; and thus it may frequently happen that serious mistakes may be made through hasty conclusions from insufficient data. It is said very truly that a really very bad water will rarely, if ever, escape condemnation, and an exceedingly pure water will undoubtedly be shown to be pure, so far as this is possible by any chemical investigation. But by far the larger number of

well waters more especially lie between these extremes, and must take their place under the head of Doubtful Purity, and it is in the judgment of the latter that the analyst is liable to error, even to the extent of rendering an opinion diametrically the opposite of that of another chemist.

One of the precautions necessary to be taken in these, and in fact in all cases, is to avoid placing any *strict* reliance, for purposes of judgment, on the standards of purity which have been published in the treatise on Water Analysis by Wanklyn, and quoted in a number of recent works on Hygiene. It is impossible to lay down exact standards, or rules for judgment, which will hold good for all countries and all localities in any one country. Such "standards" are, as Professor Nichols observes, only of relative value, and different kinds of water cannot be judged by the same standard.*

We must, first of all, discover, by numerous and carefully selected analyses, what are the chemical characteristics of good wholesome water in any given locality. These data then form a standard of purity for all waters of one kind within that particular district. A general knowledge of the geological and mineralogical character of the soil and rock of that region is an important factor in such a standard. A knowledge of the chemical character of the ground-water of the district, entirely free from any artificial conditions, such as polluted soil, is necessary for judgment of the well waters of the same district.

As regards different kinds of waters we must distinguish between

I. Ground waters, which include shallow well waters.

II. Deep well waters, including artesian wells.

III. Surface waters, such as rivers, streams, lakes and ponds.

These three classes have essentially different characteristics, and one should not be compared with another without making proper allowances for these differences. It is thus incorrect to compare well waters near Philadelphia directly with the water of the Schuylkill river, as is sometimes done.

Upon reflection it will be readily understood that well waters are not commonly subjected to the oxidixing influences to which river waters are so freely exposed, such as direct sunshine, air in motion over the surface, and aeration due to falls and currents in the river. Some mineral salts from factory refuse, from coal mines, etc., have

* Prof. W. R. Nichols' paper on "Drinking Water and Public Water Supplies," in Dr. Buck's *Hygiene*, vol. 1, page 303.

possibly a modifying influence which is absent in the case of wells. Now there is a general feeling, among the medical profession and sanitarians, that organic matter which is more liable to rapid decomposition is more dangerous to health than more stable organic substances. In river waters there are greater chances for this decomposition to have been completed, leaving such substances as may be of the latter class in much greater proportion.

Hence a larger amount of organic matter will be allowable in river water in the condition used for drinking than in well waters, provided that the supply is taken at a sufficient distance from the places where polluting material enters the stream.

Another point of importance in forming an opinion on the wholesomeness of a surface water is the natural character of its source. A river water which originates in peat bogs, or the tributary streams of which pass through a peaty district, will contain extract of peat in solution throughout its whole course. These waters certainly should not be compared, without qualification, with river waters which do not come in contact with peat, but nevertheless yield on analysis an equivalent amount of albuminoid ammonia derived from sewage contamination.

Thus, for instance, the water of Lake Cochituate, one of the sources of supply for Boston, probably contains considerable extract of peat, or other "vegetable extractive matter," which is, so far as we know, perfectly harmless in drinking water. So, also, is this the case, I believe, with the water of Jamaica Pond, which also supplies a part of Boston. Now, the Schuylkill river water supplied to Germantown, and taken from the river at Flat Rock dam, pumped at the Roxborough Water Works of Philadelphia, gave, according to my own analysis, during the winter and summer of 1878, almost exactly the same results as Professor Nichols' analysis of the Cochituate and Jamaica Pond waters in 1873 and 1875, respectively, and with the same degree of variation at different times. But, while the analytical results are the same in both cases, a comparison of the general characters and sources of these waters convinces me that we cannot call the Germantown supply nearly so pure from objectionable organic matter as the Boston water. I think we may safely say that the Schuylkill contains no extract of peat, and the albuminoid ammonia undoubtedly comes from material which is far more objectionable. At times, also, since 1878, this albuminoid ammonia is as much as one-half greater

than the largest amount from the Cochituate water in 1873, as supplied at the Massachusetts Institute of Technology, and nearly twice as great as the average amount of the latter. At such times the water has a perceptibly disagreeable taste and a slight odor, and this occurs usually when the water in the river is very low from drought.

In general, it may be said to be very necessary to know the exact source of any water submitted for analysis, the physiographical and geological characteristics of the locality from which it comes, and the location of any sources of pollution, such as cesspools, privies, drains or sewers near the place of water supply. Whatever method of analysis is used, or in whatever way performed, the necessity of a knowledge of the previous history of the water is not diminished. Even with this knowledge, as is stated by Prof. Nichols, cases may arise in which an experienced chemist will be unable to give a decided opinion.

When the river water is pumped up into distributing reservoirs, the water from near the surface, and at the bottom of these reservoirs should be submitted to analysis, so as to locate more accurately any trouble which may exist.

Furthermore, attention should be especially called to the fact previously referred to in this paper, that it is probably not so much a question as to how much organic matter, *per se*, may be consumed without danger in our drinking water. This point does enter, it is true, into the consideration. But it is *mainly* a question as to whether there is any danger of the water being contaminated with fæcal discharges from human beings suffering from infectious disease. It has been found that water containing so large a proportion of organic matter as to be called "loaded" with it may be drunk with impunity by some persons or, at least, without any disastrous result being apparent for a long time; provided it does not also contain the "contagium" or unknown "something," whatever it may be, which will of itself develop specific disease. As soon as this "something" appears to be added by a previously diseased person an epidemic of this disease breaks out among those who consumed the water and who, until this time, remained apparently healthy.

Inasmuch as we have no means whatever of discovering the presence of this "contagium" because we know nothing of its character, any contamination with sewage is dangerous in proportion to its

amount, and to the nearness of the pollution in time and place. Some authorities in Holland were once asked how much organic matter was allowable in drinking water without danger, to which they replied that drinking water should be, like Cæsar's wife, above suspicion. It may be laid down as a positive rule that a suspicious water is always a dangerous water.

The extent of dilution of the sewage in a river with a large body of moving water of good quality is undoubtedly an important factor, but this may be entirely counterbalanced by the fact of the water supply being taken very near a large sewer.

Upon these considerations lies the importance of the estimation of the nitrates and nitrites in a drinking water. These salts are among the results of the decomposition of organic nitrogenous matter, and are hence an evidence of what was either previously contained in the water, or which became oxidized by filtration through soil or by other means of decomposition before it reached the water. The greater the amount of nitrates above the natural limit, the larger the amount of organic material which has hitherto undergone oxidation, and hence the greater the danger, in the case of wells, of the soil becoming saturated and clogged with organic substance until a part of it will escape filtration and pass unchanged into the water. Moreover, this part, which has escaped oxidation, may contain, for ought we know, the poison of a specific disease. Hence, it is important in order to form a correct opinion of the sanitary character of a well water, never to omit the proper tests for nitrates and nitrites, unless all the other quantitative tests concur in pronouncing the water pure. If any one of these gives a doubtful answer, the nitrates should always be tested for, and if more than traces are present, it will be best to make a quantitative estimation of them. It is true that nitrate may, probably, also result from reduction of the true ammonia, but this fact does not render the estimation any the less important, for this ammonia itself, under ordinary circumstances, results from the decomposition of the organic matter, and in shallow wells is usually regarded as evidence of pollution with urine.

We should remember, however, that deep wells may furnish a very pure water, containing very little organic matter, but large quantities of free ammonia, chlorides, and nitrates, all present in the same sample, which are derived from certain kinds of soil, such as the sand beds beneath the London clay and about 250 feet below the surface.*

* Dr. Cornelius B. Fox. "Sanitary Examinations," p. 140.

Mr. Ekin* considers it necessary to estimate the amount of nitrates and nitrites in every instance, and states that a small increase in their amount should materially influence the judgment to be given. He states that he has been forced to this conclusion by facts ascertained in the course of his somewhat wide experience in the analysis of about 2000 samples of water, many of which were directly connected with cases of typhoid fever.

The estimation of nitrates, etc., in a river water, however, has manifestly not the same significance which it possesses in the case of well-waters, on account of the superior influences effecting oxidation in the former, and the probable absorption of nitrates by aquatic plants. As regards *nitrites*, it is stated that delicate tests have not revealed their presence in the Thames river water.

In partial corroboration of the opinions of Mr. Ekin and Dr. Fox, as to the necessity of the estimation of nitrates, I will contribute a case in my own experience. I refer to the epidemic of typhoid fever in Spring alley, or Royal street, in the southernmost part of Germantown, which occurred last summer during the last week of July and first of August. There were upwards of forty cases, including four which proved fatal; and all, except one, were grouped in the immediate neighborhood of a well situated at the intersection of two narrow streets. This well, known as the Spring alley well, has had a wide reputation as a remarkably strong pure spring for, it is said, nearly a century. It is also said that typhoid fever never occurred in this locality before 1880. The well is only ten feet deep, the ground slopes down towards it from three sides, and a brick sewer passes within about ten feet of it, at about the same depth and having two badly choked up inlets directly opposite the well. An overflow drain connected the well directly with the sewer, *entering the latter near the bottom* at a short distance beyond the well. On the 5th of July a very heavy rain occurred, which deluged the vicinity of the well. As the sewer has, at this place, scarcely any fall it must have become suddenly clogged up with filth, and backwater must have occurred through the overflow drain directly into the well, polluting it, no doubt, to a very great degree. It was subsequently found by Dr. A. F. Müller, of Germantown, that a case of typhoid fever, probably imported from elsewhere, had occurred in a neighboring house about six weeks before the heavy

* "Potable Water. How to form a Judgment on the Suitableness of Water for Drinking Purposes" By Chas. Ekin, F.C.S. London: 1880.

rain, and that the drain from this house connected with the sewer above the well. There were also other circumstances probably contributing to the pollution of this well. Another well, frightfully foul, but in no way connected with the sewer, being on higher ground, and the sewer lying between the two wells, increased, no doubt, the malignity of the epidemic. All the cases of fever developed at nearly the same time. The exceptional one, before mentioned, which did not occur close to the well, but at a house two blocks distant, was a child in a family who had sent to the first mentioned well for water, shortly after the pollution took place. All the people of the locality, and especially all those who had the fever, had used the Spring alley well, some of them continued to do so after the fever broke out, some had also used the water of the other well, but none had used the city water from the Schuylkill until after the outbreak.

The chain of evidence thus seems unusually strong and clear. My analyses of this well water were made during the prevalence of the epidemic and while new cases were developing. They are as follows:

		I.	II.	III.	IV.
Free ammonia	Parts per million,	0·034	0·010	0·010	0·058
Albuminoid ammonia		0·116	0·080	0·090	0·140
Chlorine	Grains per imp. gal.	2·3	2·6	2·3	2·4
Total solids		30·5	——	——	28·0
Nitrogen as nitrates and nitrites		——	1·94	——	2·45
Nitrogen as nitrites only		——	0·096	——	0·032

The samples were collected in the order of the numbers respectively, Aug. 10, 12, 13 and 21; the last three early in the morning and the first at noon. No. II was taken nine hours after a heavy rainfall lasting 1½ hours. No. IV was collected half an hour after a short but heavy shower. The water, nevertheless, remained quite clear. All the samples were bright and colorless, with no odor, and having a refreshing taste. The water was quite hard.

In order to judge properly of these analyses, I give, for the sake of comparison, analyses of a hard and of a soft water which are typical of the purest well waters in Germantown and perfectly free from contamination:

	Free NH_3.	Albuminoid, NH_3.	Chlorine.	Solids.
Hard water, . .	0·010	0·050	2·2	34·0
Soft water, .	0·014	0·034	0·7	6·0

The hardness in Germantown waters is due chiefly to sulphates. It will be noticed that the chlorine in the Spring alley well water was

not greater than is usual in pure hard waters here. What is still more remarkable is, that the other well in Spring alley contained not a particle more chlorine, notwithstanding that enormous amounts of free ammonia (from 2·7 to 5·2 parts per million) were present, with large excess of albuminoid ammonia, and large amounts of nitrates and considerable nitrite. In these two cases, therefore, *freedom from excess of chlorine did not prove freedom from contamination by sewage*, according to the rule usually stated. Three privies stood close to the second well, hence the chlorine test was of no value at all in this latter case. It may be added that the test was repeatedly made with the same result.

The following well waters of Germantown, which I analyzed in 1878, are examples of what we may find in wells in highly dangerous situations, which tend to confirm Mr. Ekin's statement, that dangerous wells may contain a small amount of organic matter along with considerable nitrate, and that in such cases the opinion to be rendered will depend very much on the presence of the latter:

		I.	II.	III.
Free ammonia	Parts per million,	0·040	0·022	0·016
Albuminoid ammonia,	" "	0·046	0·058	0·080
Chlorine	Grains per imp. gal.,	3·2	3·2	2·7
Nitrates		considerable.	considerable.	large am't.
Nitrites		traces.	——	——

Hardness in all these waters, about 12°, due to sulphates chiefly. The situation of the wells is as follows: No. I, Mechanic street west of Morton street, in close proximity to a number of privies which are on somewhat lower ground; soil—loose, micaceous sand. No. II, Haines street, near M. E. Church; pump on street pavement, exposed to infiltration from gutter, etc. No. III, Haines street east of Hancock street; well under kitchen porch. The owner felt anxious about it, and asked to have the water analyzed. I have not heard of any sickness having been attributed to any of these wells.

The chlorine is very slightly in excess of the amount frequently found in pure hard waters of this district, and it would, therefore, by itself, scarcely be considered a suspicious circumstance.

I will conclude this paper with the analysis by Prof. Nichols of a well water in Fairhaven, Massachusetts, published in the Massachusetts State Board of Health Report for 1879, which will be interesting in comparison with the foregoing analyses. It should also be stated that my analyses have shown that many other wells in Germantown

are *far more polluted* than this one, particularly those in crowded localities.

Well in Fairhaven, Massachusetts.

Free ammonia, . . .	0·01 parts per million.
Albuminoid ammonia, .	0·13 " "
Chlorine, ...	3·3 " 100,000.
Total solids, . . .	20·3 " "
Nitrates, ...	Not in large amount.

The privy vault was *one hundred feet* distant from the well. The soil was composed of gravel and loam. On the 7th of September the husband was taken quite ill with typhoid fever, and his dejections passed freely into the privy vault. On September 30, and during the next twelve days, his wife and six children were successively taken with typhoid fever, and another child took the same disease a few days later. Thus, every member of this one family who had used the water of the well were ill with typhoid fever. The water was probably poisoned by the excreta of the husband, and the usual incubative period intervened before the disease appeared amongst the rest of the family. The chlorine in the water was found to be one part per one hundred thousand, more than the natural amount for that locality. In order to compare it with that in the other analyses given above, muliply by $\frac{7}{10}$ to reduce to grains per imperial gallon. The date of the analysis was *October* 17, within a day or two of the development of the last case of fever.

A Great Storm.—M. Rouger has communicated to the French Academy an account of a violent storm at Laigle, in September last. Between 9.30 and 11 P.M. there were at least 4700 flashes of lightning; sometimes there were as many as three flashes per second. The rain began about 10·45 and lasted for about an hour and a half; there was no hail. The thunder was almost continuous, like a kind of buzzing, occasionally interrupted by heavy rollings. On one of the buildings that was struck there was a zinc conducting pipe, for carrying off rain water, which was pierced with three holes, two of which were square and the third round, as if made by a bullet. They all occurred where the walls of the pipe were doubled, and in each instance the burr was inward in the inner pipe and outward in the outer pipe.—*Comptes Rendus.* C.

THE BASIC DEPHOSPHORIZING PROCESS; WHAT IT IS AND WHAT MAY BE EXPECTED FROM IT.

By Jacob Reese.

A paper read before the Engineers' Society of Western Pennsylvania, Dec. 21, 1880.*

A slag is said to be basic when it is composed of metallic oxides; or, in other words, when the base of the slag is a metal the slag is said to be basic. Oxide of iron and oxide of calcium are true basic oxides, and form highly basic slags.

A slag is said to be acidulous when it is composed of a mettalloid, such as silicon, phosphorus or sulphur, oxidized to silicic acid, phosphoric acid, or sulphuric acid, and these acids combine with a base forming silicates, phosphates or sulphates. When these compounds are present in a slag in a large degree it is said to be of a highly acid character.

When a metallic process is conducted in the presence of a highly basic slag it is called a basic process, and when it is conducted in the presence of a highly acid slag it is called an acid process.

The process by which the ancient Romans and Britons made their iron is known as the Catalan process. It was conducted in an open chamber, surrounded on all sides and the bottom with a lining of charcoal dust. The fuel was used in this metal chamber admixed with the iron ore and metal. The slag present in this metal chamber, and produced by this process, was principally composed of oxide of iron; hence the Catalan was and is a basic process.

The blast furnace is lined with stone or brick, both of which are highly acidulous, and, although limestone is used as a flux, owing to the silicious character of the ore and ash of fuel the slag is of an acid character, as will be seen by the following analysis:

Silicic acid,	58
Lime,	23
Alumina,	8
Magnesia,	2
Manganese,	–
Oxide of iron,	9

Hence the blast furnace process is an acid process.

*In republishing the paper of Mr. Reese on this very important subject, this journal does not endorse the *chemistry* of the article.

Henry Cort, who invented the puddling process, was the first to separate the fuel from the metal chamber; he lined his metal chamber with sand (silicic acid); the slag was highly acidulous, and Cort's puddling process was an acid process.

Dr. Roebuck, the inventor of the refinery fire, lined the metal chamber with cast iron water boshes. The fuel was admixed with the metal. The refinery slag was highly basic, as is shown by the following analysis:

Oxide of iron,	85·0
Silicic acid,	14·0
Alumina,	0·5
Magnesia,	0·5

Hence the refinery process was a basic process.

Samuel Rodgers improved Cort's puddling process by putting an iron bottom and iron plates into the puddling furnace and lining the metal chamber with oxide of iron instead of sand. The following analysis of the slag shows it to be highly basic:

Oxide of iron,	89·0
Silicic acid,	9·0
Alumina,	1·5
Magnesia,	0·5

Hence Rodgers' puddling process is a basic process.

The Bessemer process, invented by Henry Bessemer, is conducted in a converter which is lined with ganister (a highly silicious substance). The slag produced is composed of the following:

Silicic acid,	44·30
Lime,	·65
Protoxide of manganese,	24·55
Alumina,	10·80
Magnesia,	·25
Oxide of iron,	19·45

It is a silicious slag because there is not sufficient basic material to engage all the silicic acid as silicates. And the Bessemer process is an acid process.

In the open-hearth process the metal chamber is lined with sand. When metal and scrap are used the slag is highly acidulous; and the metal and scrap open-hearth process is an acid process.

When iron ore is used in the open-hearth in considerable quantity

the slag is neutral; and the ore and metal open-hearth process may be classed with the basic processes. We therefore have as

Basic Processes.	Acid Processes.
Catalan,	Blast furnace,
Refinery,	Cort's puddling,
Rodgers' puddling,	Bessemer,
Open-hearth ore and metal,	Open-hearth metal and scrap.

The dephosphorizing problem may be summed up in these words: The phosphorus must be oxidized to phosphoric acid, P_2O_5, in the presence of a basic slag, in order that the acid so formed may unite with and be held by a metallic base as a phosphate of lime, or a phosphate of iron. And as silicic acid decomposes a phosphate of lime or a phosphate of iron, and carbonic acid decomposes a phosphate of iron, the slag must be of a highly basic character in order that all the silicic acid formed by the oxidation of silicon shall combine with and be engaged in the slag as silicates. And in order to avoid the reduction of the phosphate by carbonic acid the dephosphorization must take place in the presence of a highly basic slag, and in the absence of carbonic oxide.

In the acid processes before mentioned, as the blast furnace, Cort's puddling, Bessemer converter and the open-hearth with metal and scrap, the silicic acid and carbonic oxide reduce the phosphate to a phosphide; and as a phosphide has a greater affinity for the metal than for the slag the phosphorus is returned to the metal.

In the blast furnace, however, an additional reaction takes place under the following conditions: Carbonic oxide being always present at the zone of reduction, where the ores are deoxidized, the phosphorus accompanies the metal as a phosphide of iron. In case where the slag in the hearth does not contain over 40 per cent. of silicic acid, and does not contain 60 per cent. of basic material, the silicic acid being held as silicates, the oxide of iron contained in the slag in the free state will oxidize a portion of the phosphorus, which, uniting with oxide of iron or lime, will form a phosphate of iron or a phosphate of lime, and exist in the slag in the hearth of the furnace below the zone of carbonic oxide; under these conditions a partial dephosphorization of metal takes place in a blast furnace working on black cinder. The degree of phosphorus thus taken up by the slag will depend upon its basic character and its ability to hold the phosphorus as a phosphate.

In the old basic processes—the Catalan process and the refinery process—the fuel being admixed with the metal, carbonic oxide was always present; hence the phosphate could not exist, even in the highly basic slag, and dephosphorization was impossible. In the basic open-hearth dephosphorization takes place before the elimination of carbon commences, if the phosphoritic slag is removed, and also after the carbon has been consumed and carbonic oxide disappears.

In Rodgers' basic puddling process the chemical reactions are divided into three periods: first, the melting period; second, the boiling period; third, the solidifying period.

During the first period the metal is melted and the oxide of iron is admixed and melted. The chemical reactions which occur in this period are the oxidation of phosphorus and of silicon, and their removal from the metal to the slag. As there is no gas evolved from the oxidation of these elements the metal remains in a state of rest, except so far as agitated by the puddler's tools. If the slag be tapped off just before the close of this period it will be found to contain from 70 to 80 per cent. of the phosphorus previously contained in the pig metal. But if the slag remain with the metal until the silicon is reduced down to ·02 the second period commences, the carbon is oxidized to carbonic oxide, which, passing upward through the slag, attacks the phosphate and reduces it to a phosphide, and, as a consequence, all the phosphorus removed from the metal during the first period (and permitted to remain in the slag) is returned to the metal during the second period. As the chemical reaction during the second period is the oxidation of carbon to carbonic oxide (CO) ebullition takes place, and the metal boils. When the carbon has been reduced down to ·08 the ebullition ceases, the cinder or slag "drops" and the metal solidifies, and, as the puddlers term it, "is brought to nature" during the fluid period.

During the first part of the third period the damper is raised, and the metal which extends above the slag is exposed to an oxidizing flame. The phosphorus is either sweated out of the metal by liquidation or is oxidized by the fluid cinder, in either case, it being oxidized to P_2O_5, it again enters the slag as a phosphate of iron ($_2FeO.P_2O_5$). There being no free silicic acid or carbonic oxide in the slag during this third period the phosphorus remains in the slag until the metal is withdrawn; hence Rodgers' puddling process, when properly conducted, is a true basic dephosphorizing process, being con-

ducted in the presence of a highly basic slag, and in the absence of CO, or free silicic acid.

As phosphorus, silicon and carbon tend to reduce the fusion point of iron in degree to the amount of these elements which the metal contains, it follows that the fusion point of iron is greatly raised by the diminution of these elements, and this is the reason that the iron solidifies during the third period of the puddling process.

In the Bessemer and open-hearth processes the temperature is kept high enough to hold the metal in a fluid condition after the elimination of phosphorus, silicon and carbon, but, at the high temperature required, Rodgers' basic lining (oxide of iron) is fused also, and for this reason the Bessemer converter and the open-hearth have heretofore been lined with a silicious material.

The new basic dephosphorizing process, by which iron and steel may be desiliconized, decarbonized and dephosphorized, and yet be retained in a fluid state, so as to cast it into ingots of iron or of steel, is conducted in a metal chamber lined with lime, which is a basic material, it being the oxide of the metal calcium. By means of a lime lining the required temperature to keep the metal fluid may be employed with but little waste of the lining, and when the lining does waste, it being of a highly basic character, it tends to form a highly basic slag.

When the Bessemer converter is furnished with a lime lining, and the metal is blown in the presence of a highly basic slag until the silicon is oxidized to silicic acid, and this acid unites with bases forming silicates, and the blow is continued until the carbonic oxide disappears, and the metal is further blown in the presence of a highly basic slag, and in the absence of free silicic acid and carbonic oxide, the phosphorus is rapidly oxidized to P_2O_5, which enters the slag, and, uniting with oxide of iron, remains there as a phosphate of iron ($_2FeO.P_2O_5$), and the silicon, carbon and phosphorus are entirely eliminated from the metal. When the slag is of a highly calcareous basic character the phosphate of iron may be decomposed and a phosphate of lime formed ($CaO.P_2O_5$).

When the phosphorus exists in the slag as a phosphate of iron, if carbonic oxide be present, it will rob the phosphate of its oxygen, forming carbonic acid, and the iron and phosphorus unite as a phosphide of iron and return to the metal. But when the phosphorus exists in the slag as a phosphate of lime carbonic oxide will not reduce

it. If, however, silicic acid is present in a free state it will rob the phosphate of its base, forming a silicate of lime, and the carbonic oxide will rob the acid of its oxygen, and the phosphorus be returned to the metal.

In the practice of the basic dephosphorizing process in the Bessemer converter the silicon, carbon and phosphorus are entirely removed, and the metal is very nearly pure iron. If a small quantity of ferro-manganese or ferro-silicide is then added to partly reduce the oxygen, and the metal is cast into ingots and rolled, it will be found to possess a highly fibrous character, much superior to fibrous wrought iron made by the puddling process. And if sufficient ferro-manganese or ferro-silicide is added to thoroughly deoxidize the metal, when rolled, it will be found to possess a fine crystalline texture. In both these cases the ingot iron so made will be very ductile. When ingot steel is desired, the metal, when desiliconized, decarbonized and dephosphorized, should be removed from the slag, and then deoxidized and recarbonized in the usual manner.

When an open-hearth is furnished with a lime lining, and the metal is held at a high temperature in the presence of a highly basic bath until the silicon and carbon are eliminated and the silicic acid is engaged as silicates, and carbonic oxide disappears, and the slag is still sufficiently basic, the phosphorus is rapidly eliminated and remains in the slag as a phosphate of iron, the metal may then be treated with ferro-manganese or spiegel for deoxidizing and recarbureting it.

In both the Bessemer and the open-hearth practice it is desirable that the metal should be removed from the presence of the phosphoritic slag before the ferro-manganese or spiegel is added, as the chemical reactions, which take place when these elements are added, tend to carry a portion of the phosphorus from the slag back into the metal.

The quantity of slag required will depend on its degree of basic purity and on the amount of silicon and phosphorus to be eliminated, the essential requirement being that the slag shall possess sufficient basic material to engage all the silicic acid as silicates, and all the phosphoric acid as phosphates. To secure these requirements in excess, the weight of the slag will range from 25 to 30 per cent. of the weight of the metal.

In the economy of the new basic dephosphorizing process it is desirable to produce metal for this process containing as little silicon

as practicable, so as to prevent the appearance of silicic acid in the slag as far as possible. And as the reduction of silicon is a reduction of the source of heat, it is desirable to increase the percentage of phosphorus in the metal in proportion to the heat units withdrawn by the reduction of silicon. Therefore the most desirable metal for the new basic dephosphorizing process is that which is low in silicon, and containing from 2 to 3 per cent. of phosphorus.

When metal, containing 2 per cent. of phosphorus, is treated in a lime-lined converter, and in the presence of 25 per cent. of a highly calcareous basic slag, the slag, when withdrawn, will be found to contain 18·32 per cent. of phosphoric acid to weight of slag, or 4·58 per cent. of weight of the metal.

In order to obtain a metal from any class of ores, suitable for the new basic dephosphorizing process, containing a minimum of 2 per cent. of phosphorus, and to economize the cost of the basic calcareous slag, it is proposed to use this calcareous phosphoritic slag as a flux in the blast furnace in place of so much limestone. When this slag, containing 18·32 per cent. of phosphoric acid, is used in a blast furnace in proportion to ¼ ton of slag to 1 ton of metal produced the phosphate is reduced by the carbonic oxide and silicic acid to a phosphide, and the metal produced will contain 2 per cent. more phosphorus than was obtained from the ores from which the metal was smelted.

Thus the phosphorus, which is utilized by oxidation for the development of caloric essential to keep the metal in a highly fluid condition in the basic converter, is again utilized by its reduction in the blast furnace, securing economy and an absolute control of the production of a minimum of 2 per cent. of phosphorus in the metal made from any class of iron ores, which is desirable, as it is more convenient and practicable to eliminate 2 per cent. of phosphorus from the metal by the basic process than it is to dephosphorize a metal containing but $\frac{1}{10}$ of that amount of phosphorus and 2 per cent. of silicon.

The essential requirements of dephosphorization having been determined as set forth, *i. e.*, the oxidation of phosphorus in the presence of a highly basic slag and in the absence of free silicic acid and carbonic oxide, it is proposed to dephosphorize molten metal flowing from a blast furnace by treating it at a low temperature in the presence of a highly basic slag, and withdrawing the metal from the slag after it is dephosphorized and before the oxidation of the carbon takes place, and run-

ning the oxidized metal into pigs, or taking the metal and treating it in the acid Bessemer converter, or in an acid-lined open-hearth.

It is also proposed to dephosphorize molten metal direct from the blast furnace as before described, then to run it, minus the slag, into an open-hearth, and there desiliconize it down to ·025, and then to withdraw the dephosphorized metal and run it into pigs. Such a metal would be low in phosphorus, high in carbon and practically free from silicon, and would be a superior chilling iron for the production of chill rolls, car-wheels, and most excellent for gun metal and malleable castings, and all foundry purposes.

It is also proposed to desiliconize and decarbonize metal in the acid Bessemer converter, and then run it minus the silicious slag into a basic-lined converter or open-hearth and there dephosphorize and refine it, and then run the metal minus the phosphoritic slag into a ladle and deoxygenize and recarbonize it in the usual manner, and cast it into ingots.

Having explained the distinguishing characteristics of the old processes and the essential requisites to dephosphorization possessed by the new basic process, I will now venture a few predictions as to "what we may expect from it."

The basic dephosphorizing process will eliminate all the silicon, carbon, manganese and phosphorus contained in the metal, and produce nearly pure wrought iron.

As the fibrous character of wrought iron is caused by oxide of iron being inter-stratified in alternate lamina with the iron, and as the molten metal in the basic converter will, at the end of the blow, be somewhat oxidized if a small quantity of ferro-manganese be added to it, but not in sufficient quantity to reduce all of the oxygen, the ingot iron produced by the basic dephosphorizing process, when rolled down, will exhibit a highly fibrous texture and possess in a superior degree the properties of ductility, malleability and welding, which are possessed by the best Swedish or Norway iron—therefore the importation of such irons will cease when the basic wrought iron is put upon the market, as the latter will be better and cheaper.

As pig metal designed for the basic dephosphorizing process may be made from the cheapest class of ores, smelted in a blast furnace in which a hot blast of the highest volume and temperature is employed, the metal will be of the cheapest class produced, and as such metal, whether white, mottled or gray, can be put into fibrous wrought iron

at less cost by the new basic dephosphorizing process, than it is now, or can be, put into muck bars by the puddling process. We may expect that the history of the puddling process will be closed at an early day.

By incorporating with the basic metal just sufficient ferro-manganese to thoroughly deoxidize it, the ingot iron, when rolled down, will possess a fine crystalline texture, and yet be tough and ductile. Thus it is conceived that the fluid basic dephosphorizing process will not only produce a superior quality of steel containing any degree of carbon, chemically combined, from ·00 to 2 per cent., but it will also produce wrought iron, both fibrous and crystalline, of a quality superior to any produced heretofore by any other process. And as the degree of expansion and contraction between any given temperatures will be increased in proportion to the amount of carbon contained in a metal, and as the fluid basic dephosphorizing process is capable of producing *ingot iron free from carbon* we will be able to produce by this process a superior quality of wrought iron, possessing all the essential characteristics required for *structural purposes.*

Believing, as I do, that graphitic carbon is held in the pores of the physical structure, and that the portion of carbon, which is said to be chemically combined, is held in the pores of the chemical structure, the most pure iron, having its pores empty, when re-carbonized will take up a greater amount of carbon and exhibit greater elasticity and resilience than less pure iron so carburized. Hence we may expect to produce by the basic process *spring steel of a superior quality.*

As the most pure iron will take up the largest amount of chemically combined carbon, or, in other words, will accommodate more carbon in the chemical pores, when such iron is highly carburized, the steel will be harder, and the texture more dense; therefore we may expect to produce moldboards, landsides, plow points, and other agricultural steels of superior quality, by the basic dephosphorizing process.

As pure iron is silver white, of a very agreeable, mild, and at the same time brilliant lustre, steel produced from such iron will possess a finer texture and be capable of exhibiting a higher polish and a more beautiful lustre than iron or steel of less purity. Hence we may expect to produce steel for cutlery, cutting tools and other polished work, of a superior quality, by the basic dephosphorizing process.

The new process will produce rails of ingot iron or of any degree of carburization desired, and I believe that rails containing ·60 to ·75

of carbon, made by the basic process, will be stronger, tougher and wear double the tonnage of rails now made by the Bessemer acid process.

The basic process will produce better iron for *tin plate,* for *galvanizing,* for *stamping* and *drop forgings* than has ever been produced by any other process.

It is proposed to produce pure iron by the basic process and then re-carburize it by infusing plumbago into the metal, and thus avoid the reactions which take place in re-carburizing by the use of ferro-manganese or spiegel. Basic steel may be produced by this method possessing the peculiar characteristics of tool steel.

Pig metal for the basic process may be made from all kinds or qualities of iron ore, but for reasons before mentioned it is desirable that the metal produced shall be low in silicon, and contain not less than 2 per cent. of phosphorus. Hence by the utilization of phosphoritic ores the centre of greatest production of iron and steel may adjust itself to the economy of the basic process.

In the economy of the basic process the blast furnaces and the converters, or the open-hearths, will be located near each other, so as to save fuel, and freight on the phosphoritic slag.

The metal produced by the basic process being in a fluid condition, may be cast into ingots of any desirable shape, and as mechanical operations will be employed to work these forms in large masses the old rolling mills may not prove economical.

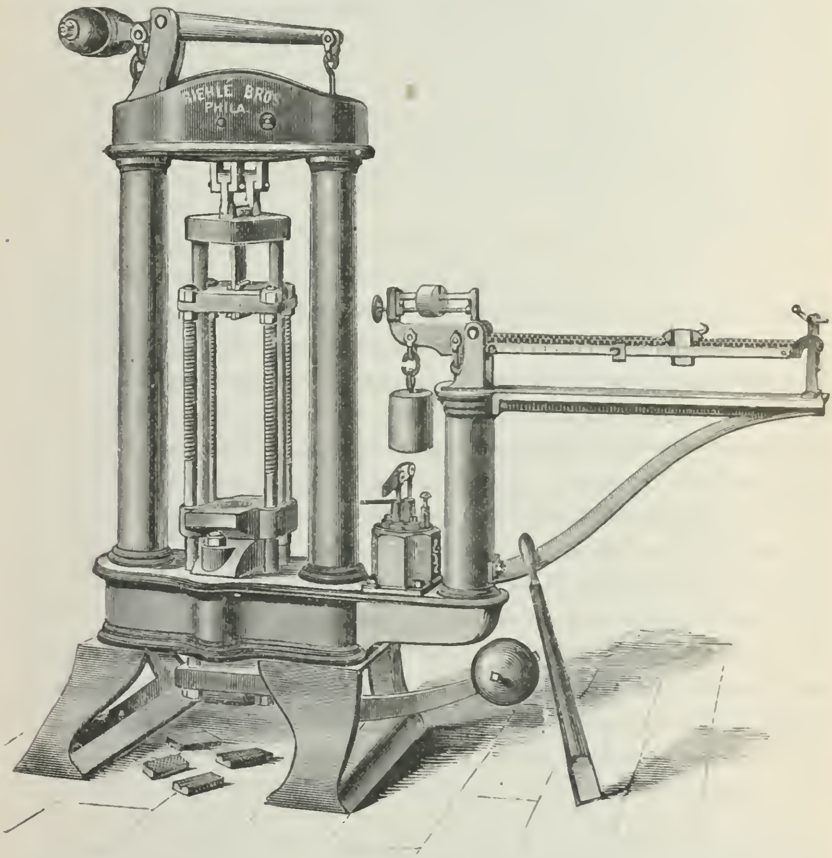
As ingots designed for plates may be cast of any desired width, and as the ingots will be reduced to plates principally by automatic process of rolling, nail plates, tank and ship plates will be produced at less cost than such are now produced by the old processes.

In conclusion, I believe that the fluid process, *i. e.,* the Bessemer and open-hearth with the basic dephosphorizing improvement, will in time supersede all others for the production of iron and steel, and will ultimately enable the United States to become the greatest exporter of iron and steel in the world.

Electric Motor.—It has been proposed to build dams upon the Seine, so as to create a water power which could be transmitted, by Siemens or Gramme machines, to different parts of Paris, and used for drawing vehicles.—*Chron. Industr.* C.

RIEHLE BROTHERS' IMPROVED VERTICAL TESTING MACHINE, 50,000 POUNDS CAPACITY.

The illustration represents one of Riehlé Brothers' improved machines for testing specimens of material, up to 50,000 pounds, by tensile, transverse and crushing strains. It is compact, strong and accurate, and well adapted for the purpose it is designed for. The shape and style are good, the various parts substantial in construction, and the workmanship unexcelled throughout the whole machine.



It is composed entirely of iron, brass and steel, and weighs about one ton. The dimensions are as follows: Extreme height, 8 feet; extreme length, 7 feet; extreme width, 2 feet 6 inches; motion of plunger of jack, 8 inches. This apparatus can test a specimen by tensile strain

from 4 or 5 inches up to 24 inches in length; transverse specimen up to 14 inches, and by the application of a superior beam to still greater length; crushing specimens, 7 inches in diameter and less, up to about 20 inches. This is a wider range of test than any machine of corresponding capacity that is made. In testing flat specimens of metal and other material with wedge grips trouble has often been experienced by the specimen slipping on or near one edge, caused by a variation in the density of the fibres of the material; this is obviated by a simple device conceived by one of the members of the firm of Riehlé Bros., namely, making the wedges slightly convex in form on their face, which at once causes the indentations of the surfaces of the wedge grips to fasten on to the test specimen, thus at once rendering the pull direct, however much the tendency may be, during the operation, of the specimen yielding to a tearing break caused by the metal naturally breaking at the weakest place, if such should exist on or near the edge of the specimen.

By referring to the engraving it will be observed that the tools which hold each end of the specimen are connected; the lower one with the power, viz., the hydraulic jack and pump, and the other with a system of multiplying levers, terminating with the weighing beam. The specimens being placed in position, the power exerted by the hydraulic pump is communicated through the specimen and the system of levers to the beam, which is kept in equipoise by the use of the poises, which are moved out, during the process of applying increased power, and consequent increased strain upon specimen, thus securing the correct registering upon the beam of the strain upon the specimen. After the test the plunger, being balanced by the lever and weight shown in the illustration, returns at once to its position before the test was made and is ready for another test. In a screw machine, the returning of the tools entails a laborious and tedious operation, almost as tiresome as the making of the test; for that reason, the public has cast its vote in favor of the hydraulic machine, especially in large machines.

Gold and Silver in Spanish Pyrites.—The quantity of silver which has been extracted in England, as an accessory product of the pyrites imported from Spain, has been 18,000 ounces, and the quantity of gold about 700 ounces. A new industry has thus arisen from a product which no one suspected a few years ago to have any merchantable value.—*La Gaceta Industrial.* C.

Circulation of Air in the St. Gotthard Tunnel.—M. Stapf has been giving careful attention to the variations in the air currents between the two openings at Göschenen and Airolo. He finds two principal causes to be operative in these changes: First, the southern opening is 30 metres (32·809 yards) higher than the northern, which represents a pressure equivalent to that of a column of air 36 metres (39·371 yards) high at the subterranean temperature; second, the difference of barometric pressures upon the two declivities of the mountain. If the external temperature was always lower than the internal, if the barometric pressure was the same at each side, and if there were no modifications of velocity due to the heating and expansion of the air or to the friction against the walls,—the draft would always be southward. Meteorological observations are regularly taken, both at Airolo and Göschenen, to determine the elements which are required in order to know, monthly or annually, the number of days for which a given direction of current or an absolute calm may be expected in the interior of the tunnel.—*Les Mondes.* C.

Fugitive Spectra.—Trouvelot, in the observations at his physical observatory in Cambridge, during the past four years has examined the sun with a spectroscope daily, whenever the weather would admit. On the 30th of August, 1877, at 2.30 P.M. he noticed for the first time a singular phenomenon. He was minutely exploring the solar circumference when suddenly, upon a group of brilliant protuberances, the solar spectrum was invaded with the rapidity of lightning and traversed by very brilliant linear spectra, which succeeded each other with great rapidity and ran through the whole visible length of the principal spectrum. He has subsequently turned his attention specially to these phenomena, and finds that they are of comparatively frequent occurrence. Their presence may be explained by two hypotheses. The first attributes them to meteors and the consequent disturbances which are supposed to give rise to the corona; the second looks to solar forces, which are brought into play by unknown causes, producing profound disturbances accompanied by eruptions of incandescent materials, either solid or liquid, which are thrown in all directions to great heights in the solar atmosphere. Trouvelot inclines to the latter view, but he is continuing his observations in the hope of finding positive evidence to show whether the bodies which produce the spectra fall into the sun, or are ejected from it.—*Ann. de Chim. et de Phys.* C.

Treatment of Boiler Incrustation.—Casalonga reports favorably upon the system of Dulac for preventing and removing incrustations. There are two distinct operations—one intended to prevent the crystallization and conglomeration of the deposits, the other to clear the water from these deposits, by means of circulatory currents passing through small boxes, which can be readily removed and in which the deposits are made. An experiment has been tried upon the boilers of MM. Raimbert and Geoffroy. The boilers were first picked and cleansed, and after twelve months' trial they were found as clean as upon the first day, the 150 kilogrammes (330·69 pounds) of mud which had been introduced in the feed water being found collected in the decanting boxes. A noteworthy fact, which shows the purity of the steam, is that the walls of the steam reservoir, the dome and the main pipe are as clean as if the metal was new. The system is said to be especially applicable to vertical boilers of all descriptions.—*Chron. Industr.* C.

Thermo-Chemical Transformations.—Berthelot has shown that every chemical change which is accomplished without the intervention of any foreign energy tends towards the production of the body or system of bodies, the formation of which sets free the greatest amount of heat. Such a final system is not always reached at once, by the first transformation of the original bodies; but it often happens that these bodies form, in the first place, new compounds, which are modified in their turn by successive steps. In order that these successive modifications may take place, it is necessary that each one of them, taken separately as well as their aggregate sum, must be accompanied by a disengagement of heat. There can be no gain of energy due to internal action alone in any of the intermediate changes. Berthelot explains in this way the various phenomena which were attributed by Schönbein to the opposite electrical polarities of ozone and antozone. He thinks that oxygenated water contains an excess of thermal energy, and this excess is gradually expended in the formation of new compounds, engendered by a suite of metamorphoses, which is impossible in bodies that have no such reserves. He regards a similar theory as applicable to a multitude of chemical phenomena, and especially to the material metamorphoses which preside over the fermentations and the nutrition of living beings.—*Ann. de Chim. et de Phys.* C.

Swiss Triangulation.—The operations in the neighborhood of Aarlberg, under the direction of the Spanish General Ibanez, for measuring the base of the network of Swiss triangulation, have been very satisfactory. The base has been measured twice. The result of the first measurement was 2400·087 metres (1·502 miles); the second operation, which was conducted entirely independently of the first, for the purpose of verification, gave 2400·085 metres. The difference between the two measurements was only two millimetres, or less than one ten-thousandth of one per cent. The place chosen for the base is on the route of Sisselen, where a perfectly straight and nearly horizontal line may be drawn for a distance of about three kilometres (1·864 miles). Similar measurements are to be made in the valleys of Tessin and of the Rhine.—*Les Mondes.* C.

Color and Liquefaction of Ozone.—Hautefeuille and Chappuis find that the isomeric transformation of oxygen into ozone under electric influence obeys simple laws. When the electric discharges cease, the mixture of oxygen and ozone ceases to be homogeneous; nevertheless it is preserved without appreciable change if a uniform temperature is maintained below the freezing point. In consequence of this relative stability of the ozone they have been able to compress the mixture and to experiment with the ozone under a pressure of many atmospheres. In increasing the pressure the capillary tube becomes of an azure blue; this color deepens in proportion to the reduction of the gaseous volume; finally the gas is of an indigo blue, and the meniscus of mercury, when seen through the gas, appears of a steel blue. The blue color of the gas becomes less intense and the mercury resumes its customary metallic aspect when the tension is diminished. Under a pressure of 75 atmospheres a thick white mist appears the moment the valve is opened. The ozonized oxygen, therefore, requires only one-fourth as great a pressure as pure oxygen, in order to secure signs of liquefaction and solidification. Ozone, however, is a little more difficult to liquefy than carbonic acid. The mixture of oxygen and ozone, containing an explosive gas, should always be compressed gradually and at very low temperatures; if these precautions are not observed the ozone decomposes, with an escape of light and heat, and there is a strong explosion, accompanied by a yellowish flash. Under a pressure of ten atmospheres the azure tint is so intense that it can be readily seen in a tube of one millimetre (·0394 inch) internal diameter.—*Comptes Rendus.* C.

Diurnal Variations of the Barometer.—G. H. Simonds has computed the barometric coefficients of daily variations from observations made at thirty stations scattered over the globe. The average duration at each station was something over ten years. Under the tropics the effect of latitude is scarcely perceptible, but between the latitudes of 20° and 60° the value of the quadrantal component decreases ·001 inch per degree of latitude. The calculations and the discussions which have resulted from them confirm Sir John Herschel's views, in regard to the universality of the phenomenon, and Chase's explanation of the cause.—*Les Mondes.* C.

Radiant Matter.—Hittorf considers that the results of Crookes' researches upon radiant matter are not essentially different from those which he himself published in 1869. The only novelty which he recognizes consists in Crookes' admission of a fourth state of aggregation. He does not regard this admission as necessary, and he is unwilling to admit that the dark space near the negative electrode represents the mean length of molecular path. He attributes the radiant matter to particles mechanically torn from the electrodes, which are charged with static negative electricity. These particles move in a straight line, with enormous velocity and affect, by a molecular electric convection, the whole track of the current between the two poles.—*Les Mondes.* C.

Electric Light in Slate Quarries.—The stability of schistose rocks is so much greater in subterranean galleries than in superficial excavations, which are exposed to the disintegrating influences of air and moisture, that there are many advantages in quarrying slates by means of mining shafts and galleries. The greatest objection to such quarries is their obscurity, which diminishes the net results of the labor and renders the proper surveillance of the walls more difficult. These inconveniences have been partly remedied by establishing regular rounds, which are constantly traversed by trustworthy watchmen; partly by replacing the old, smoky miners' lamps, in the first place by gas, and more recently by the electric light, which appears to furnish a complete solution of the problem. In the quarries of Angers all of these experiments have been successfully made, and it has been found that the electric light is about eight per cent. cheaper, and far more satisfactory than gas.—*Ann des Mines.* C.

Franklin Institute.

Hall of the Institute, January 20th, 1881.

The stated meeting was called to order at 8 o'clock P. M., the President, Mr. William P. Tatham, in the chair.

There were present 123 members and 45 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced that 14 persons were elected members of the Institute at the last meeting of the Board.

The Secretary reported the following donations to the Library:

Annual Report of the Department of Statistics and Geology of the State of Indiana, 1879.

Annual Report of the Indiana State Board of Agriculture, 1879.

Transactions of the Department of Agriculture of the State of Illinois, 1878. From L. S. Ware.

Report of Select Standing Committee on Immigration and Colonization. Canada. 1878.

University of California College of Agriculture. Supplement to the Biennial Report of the Board of Regents. 1879.

Annual Report of Massachusetts Agricultural College. 1880.

Annual Report to the Council of the City of Manchester on the Working of the Public Free Libraries. 1879–80.

From the Council.

The Gold Standard; its Causes, Effects and Future. Philadelphia: H. C. Baird & Co., 1880. From the Publishers.

Annual Report of the Supervising Inspector-General of Steam Vessels to the Secretary of the Navy, for 1880.

From the Inspector-General.

Verhandlungen des Naturhistorisch-Medicinischen Vereins zu Heidelberg. N. S. Vol. 2. Pt. 5. 1880. From the Society.

Holyoke Hydrodynamic Experiments made by Holyoke Water-power Company. 1879–80. From the Company.

Annual Report of the Commissioner of Patents for 1879.

From the U. S. Patent Office.

Act and Bull. By Lewis A. Scott.

Monetary Questions Viewed by the Light of Antiquity. By R. N. Toppan.

Proceedings of Numismatic and Antiquarian Society of Philadelphia. 1865–66.

Falsifications of Ancient Coins. By S. K. Harzfeld.

Remains of an Aboriginal Encampment at Rehoboth, Delaware. By F. Jordan, Jr. Philadelphia, 1880.

From H. Phillips, Jr., Secretary of the Numismatic Society of Philadelphia.

The Actuary read the

Annual Report of the Board of Managers..

The Board of Managers of the Franklin Institute of the State of Pennsylvania, for the Promotion of the Mechanic Arts, respectfully submits the following report for the year 1880:

Members.—During the year 165 members have been elected, and 24 have resigned.

Treasurer's Report.—The following is a condensed summary of the Treasurer's report for 1880:

Receipts.		
Balance on hand Jan. 1, 1880,	$1,405 85	
Investments of the Institute paid off,	3,000 00	
Current receipts from all other sources,	11,567 44	
		$15,973 29
Payments.		
Amounts re-invested,	$2,526 25	
All other current payments,	11,937 03	
Balance on hand Dec. 31, 1880,	1,510 01	
		$15,973 29

This statement shows our current expenses greater than the current receipts.

It is greatly to be desired that by an increase of membership this state of affairs should be reversed.

Journal.—The Journal continues under the same general management as heretofore. The financial results of the publication show that it is more than self-supporting, and it is believed that this is due to the practical value of the matter published.

The size of the monthly Journal has been increased by the addition of eight pages, making 80 pages of reading matter. Manufacturers are recognizing the Journal as an excellent means of advertising.

Library.—The gradual increase of the Library has been continued, as is exhibited by the report of your Committee on the Library.

Although new bookcases were added in 1879 and 1880, there is still not enough room for the convenient arrangement of the books, and this evil must increase until a larger library room shall be provided.

Lectures.—In the beginning of the year Mr. Charles A. Ashburner, of the Pennsylvania Geological Survey, gave two lectures on petroleum, and the course was continued by Mr. A. E. Outerbridge, Jr., on the art of coining, and the spectroscope; Dr. Robert Grimshaw, on saws; Prof. Rachel L. Bodley, on structural botany; D. S. Holman, on motion in "not living" matter; Dr. Isaac Norris, on the physical properties of metals; Hector Orr, on printing; J. B. Nicholson, on book-binding; John Sartain, on engraving; Henry Bower, on glycerine; Dr. Carl Seiler, on vocal acoustics, and Prof. Joseph Remington, on the metric system.

In the fall, the course was opened by six lectures, given by Prof. John M. Child, on the mathematics of physical science, followed by three by Dr. Seiler, on applied acoustics; lectures by Prof. Rachel L. Bodley, on household chemistry; Mr. Reuben Haines, on water; Prof. Barbeck, on microscopic botany, and Prof. E. J. Houston's holiday lecture on electricity—all of which have been largely attended.

Practically, the lectures of the Institute are now free to the public. By the resolution of the Board, seats are retained for members until five minutes to 8 o'clock, after which all who come are made welcome, and, from the interest taken in the Institute by many visiting it for the first time, the plan cannot fail to be productive of good, and a number have expressed a desire to join, and aid all in their power to enhance its future usefulness. The average attendance since the change has been about two hundred persons, while upon two occasions nearly four hundred were crowded into the hall, proving how entirely inadequate the lecture room is to accommodate all who wish to attend, and how much more good could be done if the present valuable course of lectures were delivered in a larger hall.

Drawing School.—The increase in the number of pupils applying for instruction in this department of the Institute is very gratifying to the Board. Mr. Philip Pistor, the Principal of the school, reports 96 pupils attending, and the interest they take in their studies, and the emulation excited by the Bartol scholarships, show that this important work of the Institute is properly appreciated.

The course is a progressive one, and includes instruction in mechanical, architectural and topographical drawing, both free hand and instrumental, extending over three years; but pupils, if sufficiently advanced, may select any subject of importance to them, and receive in it individual instruction. Three evenings in the week, instead of two, as heretofore, are now devoted to the school, and, as the increase of pupils is about 33 per cent. over last year, it was necessary to give notice to the Pennsylvania Museum and School of Industrial Art that the arrangement of a joint occupation of the rooms would have to cease, and they have accordingly moved elsewhere. The Board regret extremely the necessity of the action, and the severance of the very pleasant relations which have always existed between the two schools.

The facts presented in this report of the condition and work of the Institute, show that, while it is nearly self-supporting, as at present constituted, its usefulness is cramped in every department but one, by want of room in the hall which we now occupy.

The library room and the lecture room are entirely too small, and the chemical and physical laboratories attached are altogether inadequate.

The cabinet of minerals has been stored elsewhere for want of room, and the models are scattered and visible everywhere on the tops of the bookcases. The drawing school alone has room to grow.

It seems to the Board that the time has arrived for an appeal to the public for means to place this Institute in a building large enough to enable it to meet the public wants.

Unlike other institutions of similar character elsewhere, the Franklin Institute has received no appropriations of money from either State or city, yet it is altogether a public charity.

It is true that we neither feed the hungry, clothe the naked, nor heal the sick; but, on the other hand, our efforts are to dry up the sources of hunger, destitution and disease, and to avert these evils by the diffusion of such knowledge as strengthens and directs the hands of the bread winner, cheapens the cost of food and habitation, and improves the construction of dwellings.

Our charity makes no paupers; for, however much it may benefit an individual, it never diminishes his feeling of independence, for it helps those who help themselves.

The Board renews the appeal made last year to the members to aid

the Institute by adding to their number. During the past year 70 persons were nominated and elected at the instance of a single member.

By order of the Board,

W. P. TATHAM, *President.*

THE LIBRARY.

The Committee on the Library respectfully reports for the year ending December 31, 1880.

The number of volumes ordered by the committee from publishers was 81.

	Bound.	Unbound.
Number of volumes received from publishers (which includes orders given previous to 1880),	150	135
Number of volumes received for notice in Journal,	28	1
Number of volumes received as donations,	216	707
Number of volumes received as exchanges,	101	
Number of volumes received other than exchanges,	71	
Total number of volumes added in 1880,	566	843
Number of volumes in Library Dec. 31, 1879,	14,813	
Number of volumes (bound) in Library Dec. 31, 1880,	15,379	
Number of volumes repaired during the year,		24
Number of circulars received during the year,		571
Number of new exchanges ordered,		18
Number of exchanges discontinued,		14

Among the important donations were twenty volumes of the "Publication Industrielle," by Armengaud, from Mr. Frederick Graff; "Steam Boilers, their design," etc., by Schock, from Mr. B. H. Bartol; "Coney's Foreign Cathedrals," from Dr. Isaac Norris, and the "Annales Industrielles," from Mr. L. S. Ware.

A number of valuable exchanges for the Journal of the Institute, and for duplicate volumes of books in the Library, have been effected during the year.

A number of serial publications have been completed, and many books requiring it have been rebound.

The books purchased with the income from the Bloomfield Moore fund have been marked by an appropriate label, indicating the generous donation made to the Library from this source.

The number of members and other persons making use of the Library cannot be given, but the committee can report an evidently

increasing interest in, and use of, the Library. With the collection of standard works, embracing all the departments of art and science, to be found in the Library, your committee would urge upon the members their influence to extend the list of membership of the Institute, believing that in the benefit to be derived from the use of the Library will be found the full value of the annual dues for membership.

CHAS. BULLOCK, *Chairman of the Committee.*

The Secretary read memorials of the late George R. Barker and Dr. Alexander Wilcocks, and appropriate resolutions were passed.

The tellers of the election held this day presented their report, and in accordance therewith the President declared the following members elected as officers and managers:

President, William P. Tatham.

Vice-President, Charles Bullock.

Secretary, Dr. Isaac Norris.

Treasurer, Frederick Fraley.

Managers to serve three years, Prof. Pliny E. Chase, Frederick Graff, Coleman Sellers, Washington Jones, W. L. Du Bois, A. E. Outerbridge, Jr., Theodore D. Rand and Joseph M. Wilson.

Auditors, William B. Cooper and Lewis S. Ware.

The President stated that no nomination had been made at the December meeting for Trustee of the Pennsylvania Museum and School of Industrial Art, but that the by-laws governing the subject of nominations and elections did not apply to the office, which had been created since their adoption. It was competent, however, for the Institute to elect a representative at this time, and he thought it would be better to have this done.

Mr. Mitchell moved that the President be authorized to cast the vote of the Institute for Dr. Norris, saying that the latter gentleman had been a worthy representative of the Institute and entirely in harmony with the other trustees. The motion was agreed to, and the President, casting the vote, declared Dr. Norris elected.

Mr. Mitchell moved that a vote of thanks be tendered the tellers for their services, which was carried unanimously.

Among the novelties shown was Kriebel's vibrating valve engine, designed for use on farms, and by people who cannot afford to employ engineers, or to use machinery liable to get out of order and require repairs. One part of the valve vibrates with the cylinder, and is a

part of the same, while the other part is stationary and is held to the vibrating part by springs which take up the wear of the valve. The engine may be easily reversed without the use of a link, and a movable plate between the valve surfaces enables the engineer to adjust the cut-off to any desired point. Engines of this type have been in use in Texas for more than two years. The chief claim of the inventor is simplicity of construction.

Mr. Kriebel, by a photograph projected upon the screen, explained the construction of the valve, and exhibited the engine in operation, and showed how readily it could be reversed.

Mr. Orr inquired as to the power of the engine exhibited, and was told that it was eight horse-power. In answer to other inquiries Mr. Kriebel stated that the earlier engines of this type were built in Texas, but that this particular engine was not.

Dr. Norris read a paper describing Mr. Thomas S. Speakman's proposed tidal motor, and the latter gentleman exhibited a model of the apparatus.

Mr. Speakman's paper describing his proposed motor reviewed previous attempts made in the same direction, but in which only the flow of the tide was utilized. His proposed motor is designed to use the vertical force of the ebb and flow of the tide. A reservoir in which the tide water flows is provided with a float, which rises and falls with the tide, and its movement is utilized in driving the piston of a pump. At this city, the tide rises about six feet, and Mr. Speakman claims that he could obtain four strokes per day of the piston of a pump lifting a column of water to that height. The design is to use very large floats and proportionately large pumps. It is also proposed to connect the float with a shaft by means of ratchets, and to raise the speed of machinery driven by this motor to any desired extent by the use of ordinary devices of shafting and gearings. Mr. Speakman presented calculations of the power of the motor with floats and pistons of different diameter, and claimed that the motor would be economical, as the running expense would be trifling in amount.

In answer to questions by Messrs. Tatham, Cooper and others, Mr. Speakman said that the piston was packed tightly, that it would make four strokes a day (that is, two double strokes), that the cost of the power derived was almost nothing beyond interest on the original cost of the apparatus, and that no attempt had been made to reduce the effects of the machine to horse-power.

Dr. Robert Grimshaw made a few remarks upon "Modern Milling." He said that within the last ten years milling had advanced to be a true science, requiring higher knowledge of the art than formerly, guided and aided by principles not laid down until within the last ten years.

The new process milling simply means reversing the entire miller's art; taking what was formerly thrown away to make a high grade. There are five or six different systems, and the changes from the old system are very marked, as mills are now built which make 4000 barrels of flour per day.

The changes are in almost every branch of milling, in the parties employed, in the material used, in the process, in the product, in the location of the milling industry, in the habit of grinding, in the size of the mills, in the power by which the mills are universally driven, and the importance of the industry, and it has been lifted to a noble and useful art. The new process flour is not made from the flour itself, but from the broken bits of the berry. The wheat ground between stones was broken up into bran, sharps and loose flour. The proportion of sharps was about 12 or 15 per cent. In the new process milling, in any one of the five systems, they aim to produce from 75 to 80 per cent. of middlings. Instead of using the old grinding, granulation is now used. There are now employed five different classes of machines in granulation, and in their subdivision about 22 different classes of machines.

The speaker then had thrown upon a screen pictures showing different kinds of mill machinery, and explained, as he went along, their action and advantages. He also had samples of flour in all stages of milling, from the grain of wheat to the finished article ready for the baker's use.

In speaking of the third class of mill machinery, which he characterized as that by which the best part of our American flour is made, he said that the rollers might be either made of porcelain, stone, corrugated iron, smooth iron, or steel, and that they might be run in pairs or single. He also said that to-day Minnesota and Dakota wheat were taking the lead in the new process. After showing the different classes of machines, he also had cast upon the screen representations of the berry, showing how the machine treated it.

Upon motion, the Institute adjourned.

Isaac Norris, M. D., *Secretary.*

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ON THE EXPERIMENTS WITH THE PERKINS MACHINERY OF THE STEAM YACHT "ANTHRACITE."

By Chief-Engineer ISHERWOOD, United States Navy.

(Continued from page 99.)

THE ENGLISH EXPERIMENT WITH THE "ANTHRACITE."

The experiment made in England with the *Anthracite* in free route, was conducted by Mr. Bramwell, whose report is before me together with *fac similes* of all the indicator diagrams taken on that occasion, and the working drawings of the cylinders, valve chest, etc. The text of the report is exceedingly meagre, giving but little information beyond the fact of the number of pounds of coal per hour for which the indicated horse-power was obtained. Neither the manner of conducting the experiment, nor of reporting it, is comparable, in sagacity of method and completeness of data and results, with the New York Navy Yard experiment and report made by Chief Engineer Loring, U. S. N., and were it not for the possession of the indicator diagrams and of the working drawings of the cylinders above referred to, but little could be elicited from Mr. Bramwell's report of value to the engineer.

Mr. Bramwell's method was to commence with the boiler and its water cold, weigh all the coal, including the coal equivalent of the

kindling wood, consumed from the starting of the fire, and, at the close of the experiment, burn the fire entirely out, working the machinery and taking the diagrams until the engine stopped for want of steam. The work done during this time was computed in foot-pounds from the diagrams, which work could be easily turned into indicated horses-power. This method was unfair to the machinery. The heating of the boiler and its contained water to the working temperature is not properly chargeable to the performance of the engine, nor is it exact to average the declining power of the engine during the burning out of the fire with its proper performance at normal pressure. As the steam pressure sunk with the sinking fire during the latter part of the experiment, the back pressure against the piston remaining constant, the indicated pressure became continuously a less and less proportion of the total pressure on the pistons with a corresponding falling off in the economic result.

The proper manner was to commence the experiment with the fire in normal condition for steady action, and to end with it in the same state, maintaining the steam pressure and rate of combustion as uniform as possible throughout, and continuing the experiment sufficiently long to render any error in the valuation of the condition of the fire at the beginning and end practically insensible when spread over such an extent of time.

Among the omissions in the report is the height of the barometer, which has been supplied by assuming it at the standard of 29·92 inches of mercury. The height of the barometer is indispensable for obtaining the pressures above zero from the indicator diagrams, and without these pressures the distribution of the steam in the cylinders cannot be known. The information to be thus obtained from these diagrams is very important, and is much in addition to merely the indicated pressure which is usually, as in Mr. Bramwell's report, all that is derived from them. The pressure above zero at the commencement of the stroke of the piston, at the point of cutting off the steam, and at the end of the stroke of the piston; the mean back pressure against the piston during its stroke, and the back pressure at the commencement of its stroke, should always be given as well as the indicated pressure.

The refuse from the coal is not stated, but as the fuel used was Nixon's navigation coal, the refuse from which is known by other experiments to be about 5 per centum, that proportion has been

assumed. This coal is the finest steam coal in the world, and is used exclusively in experiments made for the British Admiralty. As it comes from the mine it has but little refuse, and that little is afterwards lessened by careful hand picking. It produces neither smoke nor clinker, the refuse being a small quantity of light white ash. It is free burning to a remarkable degree, does not cake, intumesce or cohere, and requires but little cleaning of the fire. The pound of its combustible matter, that is, a pound of what remains after deduction of the refuse, has, undoubtedly, a higher water vaporizing value in a boiler than the pound of combustible matter from any other coal known.

No temperatures are given in the report, not even the temperature of the feed water, which has been assumed the same as during the Navy Yard experiment, the vacuum in the condenser being very nearly alike in both cases.

The waste spaces in the steam passages and clearances at the ends of the cylinders are not given in the report, although their disproportionately great extent materially affects opinion on the performance. No statement is made of where the water level was carried, nor is there given the proportion of the boiler surface in water and in steam. The text of the report, indeed, furnishes no means of intelligently understanding the performance, or of analyzing it; but its omissions have been supplied from the other sources, so that the complete data and results are presented in the following table with probably sufficient accuracy for practical purposes.

The weight of water pumped into the boiler during the English experiment was not ascertained; it is a very important quantity and might easily have been measured in the same manner as during the Navy Yard experiment, the quantity per hour being so small as to be quite manageable. In the following table, however, this quantity is given, and it was thus deduced. From the Navy Yard experiment the number of pounds of steam condensed per hour in the third cylinder at the end of the stroke of its piston by causes other than the production of the power, was experimentally ascertained, but as the extreme temperatures in this cylinder during the two experiments were different, the difference being less in the English trial, the Navy Yard condensation had to be diminished in the ratio of the two differences. To the quantity so obtained were added the number of pounds of steam accounted for per hour by the indicator at the end of the stroke

of the piston of the third cylinder, and the sum is the number of pounds of feed water pumped into the boiler per hour. This quantity is indispensable for understanding and reconciling the differences in the economic results obtained during the two experiments; the error in it, if there be any, cannot exceed one or two per centum.

The length of the trial has been taken at 11 hours and 10 minutes, as for that time only can the coal consumption be recovered for the proper conditions of the trial. The indicator diagrams taken half-hourly during this time have all been calculated, and the mean number of double strokes made by the pistons of the engine for the same time has also been found. The mean steam pressure in the boiler and in the receiver during this time is also ascertained, as well as the mean vacuum in the condenser. The consumption of coal was obtained as follows:

From 7.22 A.M. until 6.30 P.M., a period of 11 hours and 10 minutes consecutively, the engine operated with great uniformity of conditions, the steam pressure in the boiler varying from 340 pounds per square inch above the atmosphere as the minimum to 370 pounds as the maximum, the mean being 357 pounds per square inch above the atmosphere. The quantity of coal consumed during this period is not stated in the report, but the statement is made therein that from 7.50 A.M. until 4.45 P.M., a period of 8 hours and 55 minutes comprised within the above, 1232 pounds of coal were thrown into the furnace. If the assumption be now made that the condition of the fire was the same when the last of this coal was thrown in as it was when the first was thrown in, which was undoubtedly the case judging from the uniformity of the steam pressure and number of double strokes made by the pistons before and after as well as during the period of 8 hours and 55 minutes, then the 1232 pounds of coal were consumed in the 8 hours and 55 minutes, or at the rate of 138·168224 pounds per hour, or 9·0213 pounds per hour per square foot of grate surface, which, for the kind of coal used, was an extremely slow combustion. The mean number of double strokes made per minute by the pistons of the engine during the above 11 hours and 10 minutes, was 130·3881, and is exactly the same as during the 8 hours and 55 minutes, the mean steam pressure in the boiler being also almost exactly the same during the two periods. The above rate of combustion of 138·16824 pounds of coal per hour, according to the report, during

the 8 hours and 55 minutes, may therefore be considered the mean rate of combustion for the entire period of 11 hours and 10 minutes.

Table containing the Data and Results of the Experiment made in England by Mr. Bramwell on the Machinery of the Steam Yacht ANTHRACITE *to determine its Economic Performance:*

	Date of the experiment (vessel in free route),	May 22, 1880.
	Number of sets of indicator diagrams, taken half-hourly,	22
TOTAL QUANTITIES.	Duration of the experiment in hours and minutes, consecutively,	11·10
	Total number of pounds consumed of Nixon's navigation steam coal,	1542·8785
	Total number of pounds of refuse in ash, etc., from the coal,	77·1439
	Total number of pounds of combustible (gasifiable portion of the coal) consumed,	1465·7346
	Per centum of the coal in refuse of ash, etc.,	5·
	Total number of double strokes made by the pistons of the engine,	87360·
ENGINE.	Steam pressure in the boiler, in pounds per square inch above the atmosphere,	357·0
	Steam pressure in the receiver, in pounds per square inch above the atmosphere,	9·4
	Position of the throttle valve,	Partly closed.
	Fraction completed of the stroke of the piston of the 1st cylinder when the steam was cut off,	0·4887
	Fraction completed of the down stroke of the piston of the 3d cylinder when the steam was cut off,	0·2957
	Fraction completed of the up stroke of the piston of the 3d cylinder when the steam was cut off,	0·3222
	Number of times the steam was expanded,	26·8851
	In none of the cylinders was the steam cushioned, nor was there either steam or exhaust lead.	
	Vacuum in the condenser, in inches of mercury,	26·864
	Back pressure in the condenser in pounds per square inch above zero,	1·501
	Probable temperature in degrees Fahrenheit, of the feed water,	122·
	Number of double strokes made per minute by the steam pistons,	130·3881
	Temperature, in degrees Fahrenheit, of the boiler steam, considered as saturated,	430·48
	Temperature, in degrees Fahrenheit, of the steam in the 1st cylinder at the commencement of the stroke of the piston, considered as saturated,	385·28

Rate of Combustion.	Pounds of coal consumed per hour,	138·1682
	Pounds of combustible consumed per hour,	131·2598
	Pounds of coal consumed per hour per square foot of grate,	9·0213
	Pounds of combustible consumed per hour per square foot of grate,	8·5702
	Pounds of coal consumed per hour per square foot of outer heating surface,	0·4602
	Pounds of coal consumed per hour per square foot of inner heating surface,	0·6127
	Pounds of combustible consumed per hour per square foot of outer heating surface,	0·4372
	Pounds of combustible consumed per hour per square inner heating surface,	0·5821
Steam Pressure in 1st Cylinder per Indicator.	Pressure on piston of 1st cylinder at commencement of its stroke, in pounds per square inch above zero,	205·03
	Pressure on piston of 1st cylinder at the point of cutting off the steam, in pounds per square inch above zero,	181·51
	Pressure on piston of 1st cylinder at the end of its stroke, in pounds per square inch above zero,	105·26
	Mean back pressure against piston of 1st cylinder during its stroke, in pounds per square inch above zero,	41·23
	Back pressure against piston of 1st cylinder at commencement of its stroke, in pounds per square inch above zero,	32·30
	Indicated pressure on piston of 1st cylinder, in pounds per square inch,	124·546
	Net pressure on piston of 1st cylinder, in pounds per square inch,	122·546
	Total pressure on piston of 1st cylinder, in pounds per square inch,	165·776
Steam Pressures in 2d Cylinder per Indicator.	Pressure on piston of 2d cylinder at commencement of its stroke, in pounds per square inch above zero,	62·33
	Pressure on piston of 2d cylinder at the end of its stroke, in pounds per square inch above zero,	29·83
	Mean back pressure against piston of 2d cylinder during its stroke, in pounds per square inch above zero,	25·94
	Back pressure against piston of 2d cylinder at commencement of its stroke, in pounds per square inch above zero,	24·10
	Indicated pressure on piston of 2d cylinder, in pounds per square inch,	15·606
	Net pressure on piston of 2d cylinder, in pounds per square inch,	13·606
	Total pressure on piston of 2d cylinder, in pounds per square inch,	41·456

Steam Pressures in 3d Cylinder per Indicator, Down Stroke.	
Pressure on top of piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero, ...	20·06
Pressure on top of piston of 3d cylinder at the point of cutting off the steam, in pounds per square inch above zero, ...	16·77
Pressure on top of piston of 3d cylinder at the end of its stroke, in pounds per square inch above zero, .	7·02
Mean back pressure against top of piston of 3d cylinder during its stroke, in pounds per square inch above zero, ...	4·05
Back pressure against top of piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero, ...	3·64
Indicated pressure on top of piston of 3d cylinder, in pounds per square inch, ...	8·252
Net pressure on top of piston of 3d cylinder, in pounds per square inch, ...	6·252
Total pressure on top of piston of 3d cylinder, in pounds per square inch, ...	12·302
Weight of Steam Accounted for by the Indicator.	
Pounds of steam present per hour in the 1st cylinder at the point of cutting off the steam, calculated from the pressure there, ...	689·3756
Pounds of steam present per hour in the 1st cylinder at the end of the stroke of its piston, calculated from the pressure there, ...	590·6485
Pounds of steam condensed per hour in the 1st cylinder to furnish the heat transmitted into the total horses-power developed in that cylinder by the expanded steam alone, ...	45·1659
Sum of the two immediately preceding quantities,	635·8144
Pounds of steam present per hour in the 2d cylinder at the end of the stroke of its piston, calculated from the pressure there, ...	1004·7334
Pounds of steam condensed per hour in the 1st and 2d cylinders to furnish the heat transmitted into the total horses-powers developed in those cylinders by the expanded steam alone, ...	124·8752
Sum of the two immediately preceding quantities, .	1129·6086
Pounds of steam present per hour in the 3d cylinder at the end of the stroke of its piston, calculated from the mean of the pressures there for the down stroke and the up stroke of the piston, ...	1118·3780
Pounds of steam condensed per hour in the 1st, 2d and 3d cylinders to furnish the heat transmitted into the total horses-power developed in those cylinders, by the expanded steam alone, ...	199·1154
Sum of the two immediately preceding quantities,	1317·4934

STEAM PRESSURES IN 3D CYLINDER PER INDICATOR, UP STROKE.	Pressure on bottom of piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero,	21·94
	Pressure on bottom of piston of 3d cylinder at the point of cutting off the steam, in pounds per square inch above zero,	17·55
	Pressure on bottom of piston of 3d cylinder at the end of its stroke, in pounds per square inch above zero,	7·66
	Mean back pressure against bottom of piston of 3d cylinder during its stroke, in pounds per square inch above zero,	4·44
	Back pressure against bottom of piston of 3d cylinder at commencement of its stroke, in pounds per square inch above zero,	4·05
	Indicated pressure on bottom of piston of 3d cylinder, in pounds per square inch,	10·247
	Net pressure on bottom of piston of 3d cylinder, in pounds per square inch,	8·247
	Total pressure on bottom of piston of 3d cylinder, in pounds per square inch,	14·687
HORSES-POWER.	Indicated horses-power developed in the 1st cylinder,	29·0173
	Indicated horses-power developed in the 2d cylinder,	14·6746
	Indicated horses-power developed in the 3d cylinder (down stroke of piston),	16·6570
	Indicated horses-power developed in the 3d cylinder (up stroke of piston),	20·3834
	Aggregate indicated horses-power developed in all the three cylinders,	80·7323
	Net horses-power developed in the 1st cylinder,	28·5514
	Net horses-power developed in the 2d cylinder,	12·7940
	Net horses-power developed in the 3d cylinder (down stroke of piston),	12·6199
	Net horses-power developed in the 3d cylinder (up stroke of piston),	16·4050
	Aggregate net horses-power developed in all the three cylinders,	70·3703
	Total horses-power developed in the 1st cylinder,	38·6233
	Total horses-power developed in the 2d cylinder,	29·3869
	Total horses-power developed in the 3d cylinder (down stroke of piston),	13·2643
	Total horses-power developed in the 3d cylinder (up stroke of piston),	15·4050
	Aggregate total horse-power developed in all the three cylinders,	96·6795
	Total horses-power developed by the expanded steam alone in the 1st cylinder,	15·6233
	Total horses-power developed by the expanded steam alone in the 2d cylinder,	29·3869
	Total horses-power developed by the expanded steam alone in the 3d cylinder,	28·6693

Weight of Water Vaporized in the Boiler from the Feed Temperature.	Pounds of steam evaporated per hour in the boiler on the supposition that this weight was equal to the weight of steam accounted for by the indicator at the end of the stroke of the piston of the 3d cylinder plus 121·9992 pounds condensed in that cylinder by other causes than the development of the power; this 121·9992 pounds is calculated from the weight of 147·2538 pounds condensed per hour in the 3d cylinder during the experiment made at the New York Navy Yard on the machinery of the *Anthracite*, divided by the ratio 1·207 of the difference between the temperatures of the initial steam in that cylinder on its piston and of the back pressure steam against it at the commencement of the stroke in that experiment and in the present one. In the Navy Yard experiment the temperature of the initial steam on the piston of the 3d cylinder was 245·76 degrees Fahrenheit, and the temperature of the minimum back pressure against that piston was 150·25 degrees Fahrenheit; difference 95·51 degrees Fahrenheit. In Mr. Bramwell's experiment the temperature of steam of the initial pressure on the piston of the 3d cylinder was 230·60 degrees Fahrenheit, and the temperature of the minimum back pressure against it was 151·47 degrees Fahrenheit; difference 79·13 Fahrenheit. And $\frac{95\cdot51}{79\cdot13} = 1\cdot207$, the ratio used above, ...	1439·4926
Economic Results.	Pounds of coal consumed per hour per indicated horse-power, ...	1·7111
	Pounds of coal consumed per hour per net horse-power,	1·9631
	Pounds of coal consumed per hour per total horse-power,	1·4291
	Pounds of combustible consumed per hour per indicated horse-power, ...	1·6259
	Pounds of combustible consumed per hour per net horse-power, ...	1·865[illegible]
	Pounds of combustible consumed per hour per total horse-power, ...	1·3577
	Pounds of feed water consumed per hour per indicated horse-power, ...	17·8301
	Pounds of feed water consumed per hour per net horse-power, ...	20·4560
	Pounds of feed water consumed per hour per total horse-power, ...	14·8895
	Fahrenheit units of heat consumed per hour per indicated horse-power, ...	2021·7027
	Fahrenheit units of heat consumed per hour per net horse-power, ...	2295·9810
	Fahrenheit units of heat consumed per hour per total horse-power, ...	16719·1505

DIFFERENCE BETWEEN THE WEIGHT OF WATER VAPORIZED IN THE BOILER AND THE WEIGHT OF STEAM ACCOUNTED FOR BY THE INDICATOR.	Difference in pounds per hour, between the weight of water vaporized (1439·4926 pounds) in the boiler and the weight of steam accounted for by the indicator in the 1st cylinder at the point of cutting off the steam,	450·1170
	Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 1st cylinder at the point of cutting off the steam, . .	31·27
	Difference in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 1st cylinder at the end of the stroke of its piston, . . .	503·6782
	Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 1st cylinder at the end of the stroke of its piston, . .	34·99
	Difference in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 2d cylinder at the end of the stroke of its piston, . . .	309·8840
	Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 2d cylinder at the end of the stroke of its piston, . .	21·53
	Difference in pounds per hour, between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator in the 3d cylinder at the end of the stroke of its piston, . . .	121·9992
	Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the 3d cylinder at the end of the stroke of its piston, . .	8·47
BOILER VAPORIZATION.	Number of pounds of water that would have been vaporized in the boiler per hour had the feed water been supplied at the temperature of 100 degrees Fahrenheit and vaporized under the atmospheric pressure of 29·92 inches of mercury, ...	1498·7042
	Number of pounds of water that would have been vaporized in the boiler per hour had the feed water been supplied at the temperature of 212 degrees Fahrenheit and vaporized under the atmospheric pressure of 29·92 inches of mercury, ...	1673·8137
	Pounds of water vaporized from 100 degrees Fahrenheit by one pound of coal, ...	10·8469
	Pounds of water vaporized from 100 degrees Fahrenheit by one pound of combustible, . . .	11·4179
	Pounds of water vaporized from 212 degrees Fahrenheit by one pound of coal, ...	12·1143
	Pounds of water vaporized from 212 degrees Fahrenheit by one pound of combustible, . . .	12·7519

PER CENTUM OF TOTAL PRESSURE ON PISTONS UTILIZED AS INDICATED AND AS NET PRESSURES.	Mean indicated pressure on the piston of the 3d cylinder, equivalent to the sum of the indicated pressure on that piston and of the indicated pressures on the pistons of the 2d and 1st cylinders reduced respectively in the ratio of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and for the fact of the 2d and 1st cylinders being single acting, while the 3d cylinder is double acting, in pounds per square inch, ...	20·1464
	Mean total pressure which applied to the piston of the 3d cylinder would produce the total horses-power developed by the engine, provided the indicated pressure on that piston was the above 20·1464 pounds per square inch, ...	24·1231
	Per centum of the mean total pressure on the pistons of the three cylinders utilized as indicated pressure, .	83·50
	Mean net pressure on the piston of the 3d cylinder, equivalent to the sum of the net pressure on that piston and of the net pressures on the pistons of the 2d and 1st cylinders reduced respectively in the ratio of the areas of the pistons of the 2d and 1st cylinders to that of the 3d cylinder, and for the fact of the 2d and 1st cylinders being single acting, while the 3d cylinder is double acting, in pounds per square inch, . .	17·5608
	Per centum of the mean total pressure on the pistons of the three cylinders utilized as net pressure, . .	72·80

Comparison of the Two Experiments.

The most cursory glance at the results of the experiment made on the machinery of the *Anthracite* by the Board of Naval Engineers at New York, and by Mr. Bramwell in England, shows a wide difference, and it will be interesting and instructive to trace the cause and to reconcile the discrepancy. This seems a difficult task when the indicated horse-power cost in the former experiment 2·7115 pounds of coal per hour, and in the latter experiment 1·7114 pounds, yet this chasm can be bridged.

Referring, now, to the tables containing the data and results of the experiments, it will be seen from the quantities grouped under the head of "difference between the weight of water vaporized in the boiler and the weight of steam accounted for by the indicator" that the proportion which the latter is of the former is greatly less in the experiment made by the Naval Engineers than in Mr. Bramwell's experiment; in other words, the cylinder condensation was much

greater in the Engineers' experiment than it was in Mr. Bramwell's. The following are the exact figures for the periods stated.

	Experiment by Board of Naval Engineers.	Experiment by Mr. Bramwell.
Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the first cylinder at the point of cutting off the steam,	56·76	31·27
Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the first cylinder at the end of the stroke of its piston,	56·22	34·99
Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the second cylinder at the end of the stroke of its piston,	38·41	21·53
Difference in per centum of the weight of water vaporized in the boiler, between that weight and the weight of steam accounted for by the indicator in the third cylinder at the end of the stroke of its piston,	10·05	8·47

In the Engineers' experiment the pressure of the steam at the commencement of the stroke of the piston of the first cylinder was 201·64 pounds per square inch above zero, and in Mr. Bramwell's experiment it was 205·03 pounds. The back-pressure against the piston of the third cylinder was very nearly the same in both experiments, so if the steam was in the same condition as regards superheating in both experiments, the difference between the extreme temperatures in the cylinders in both cases was insignificant, and this difference is a measure—other things equal—of the cylinder condensation. But so far from other things being equal, there were great inequalities during the two experiments in two important particulars. 1st. The steam, though greatly throttled in the Engineers' experiment by the small cross area of the steam pipe, notwithstanding the throttle valve was kept wide open, was much more throttled in Mr. Bramwell's experiment by the combined small area of the steam pipe and the partial closing of the throttle valve. The throttling in the first case caused a reduction of the boiler pressure of 129·59 pounds per square inch when the steam entered the first cylinder, and in the second case caused

a similar reduction of 166·66 pounds per square inch. The superheating in the first case, due to the difference between the temperature of the steam in the boiler and its temperature in the first cylinder at the beginning of the stroke of its piston, was 35 degrees Fahrenheit, and in the second case the similar superheating was 45 degrees Fahrenheit. In the second case, also, the engine was supplied, owing to the greater throttling, with drier steam, independently of the superheating. Both the causes just stated operate to lessen the cylinder condensation more in Mr. Bramwell's than in the Engineers' experiment.

2d. In Mr. Bramwell's experiment the pistons of the engine made 130·3881 double strokes per minute, while, in the Engineers' experiment, they made only 103·02782 double strokes. The initial, final and mean pressures on the pistons were nearly the same in both experiments, the greater number of double strokes of pistons in equal time made during Mr. Bramwell's experiment being due to the fact that the vessel was then in free route, while during the Engineers' experiment she was held stationary to the dock. Now, with *the just mentioned pressures on the pistons maintained constant*, the weight of steam consumed per hour by the engine would be in the ratio of the number of double strokes made by the pistons, while the weight of steam condensed per hour in the cylinder would remain sensibly constant, the interior surfaces of the cylinder being in both cases half the time exposed to the steam temperature and half the time to the exhaust temperature, so that although the cylinder condensation remained constant in weight of steam condensed per hour, yet this weight becomes a less and less proportion of the weight of steam evaporated per hour in the boiler as the speed of the piston becomes greater and greater; hence, there is an economic gain with increased speed of piston, *the piston pressure remaining constant.* These remarks, however, apply only when cylinder condensation exists, and in proportion to its extent, the gain being due wholly to the lessening of the per centum of that condensation. When no such condensation exists the cost of the power in heat is unaffected by the speed of the piston.

The per centum of cylinder condensation should, owing to the above causes, be certainly less in Mr. Bramwell's than in the Engineers' experiment, and the determinations show the difference to be very considerable, as might be expected from the great percentage of this condensation in the Engineers' experiment. For example, the difference

between the weight of steam condensed at the point of cutting off in the first cylinder is, in the two experiments, (56·76 — 31·27 =) 25·49 per centum of the weight evaporated in the boiler. At the end of the stroke of the piston of the first cylinder it is (56·22 — 34·99 =) 21·23 per centum. At the end of the stroke of the piston of the second cylinder it is (38·41 — 21·53 =) 16·88 per centum. And at the end of the stroke of the piston of the third cylinder it is (10·05 — 8·47 =) 1·58 per centum.

There must now be examined whether the difference in the per centum of the cylinder condensations in the two experiments will account for the difference in the heat cost of the powers developed respectively.

In the first cylinder, during the Engineers' experiment, there were utilized of the total weight of steam evaporated in the boiler (100·00 — 56·22 =) 43·78 per centum; but if the cylinder condensation had been the same per centum as during Mr. Bramwell's experiment the percentage utilized would have been (100·00—34·99=) 65·01, so that the total horses-power developed in that cylinder during the Engineers' experiment, instead of being 28·8902, would have been $\left(\frac{28·8902 \times 65·01}{43·78}=\right)$ 42·9000.

In the second cylinder, during the Engineers' experiment, there were utilized of the total weight of steam evaporated in the boiler (100·00 — 38·41 =) 61·59 per centum; but if the cylinder condensation had been the same per centum as during Mr. Bramwell's experiment the percentage utilized would have been (100·00—21·53=) 78·47, so that the total horses-power developed in that cylinder during the Engineers' experiment, instead of being 23·2490, would have been $\left(\frac{23·2490 \times 78·47}{61·59}=\right)$ 30·8949.

In the third cylinder, during the Engineers' experiment, there were utilized of the total weight of steam evaporated in the boiler (100·00 — 10·05 =) 89·95 per centum; but if the cylinder condensation had been the same per centum as during Mr. Bramwell's experiment the percentage utilized would have been (100·00—8·47=) 91·53, so that the total horses-power developed in that cylinder during the Engineers' experiment, instead of being 28·0135, would have been $\left(\frac{28·0133 \times 91·53}{89·95}=\right)$ 28·5054.

The sum of the above new total horses-power is (42·9000 + 30·8919 + 28·5054 =) 102·3003, which is what the engine would have developed during the Engineers' experiment with the weight of steam then evaporated in the boiler, had the cylinder condensation been the same per centum as in Mr. Bramwell's experiment. Dividing the weight of steam evaporated in the boiler during the engineers' experiment by the new total horses-power, there results for the cost of the total horse-power $\left(\frac{1465 \cdot 11822}{102 \cdot 3003} = \right)$ 14·3217 pounds of feed-water per hour, equivalent to (14·3217 × 1121·4 =) 16060·3992 Fahrenheit units of heat. In Mr. Bramwell's experiment the total horse-power cost 14·8893 pounds of feed water per hour, equivalent to 16719·1503 Fahrenheit units of heat, the discrepancy being about 4 per centum of the greater quantity, a very close approximation, which includes all errors of observation, calculation and assumption. It is therefore clear that the difference in the cost of the power during the two experiments was wholly due to and measured by the difference of the cylinder condensations, and no other reason can be given why the condensations should have differed so widely than the less amount of superheating possessed by the boiler steam in one experiment than in the other, and the greatly less speed of piston with equal piston pressures.

Taking, in the case of the two experiments, the number of Fahrenheit units of heat consumed per hour per total horse-power for the measure of the economic results, and assuming that cost in the engineers' experiment as unity, the performance of the machinery in Mr. Bramwell's experiment was $\left(\frac{20498 \cdot 22 - 16719 \cdot 15 \times 100}{20498 \cdot 22} = \right)$ 18·346 per centum superior. This 18·346 per centum was the measure of the gain obtainable from the greater degree of superheating possessed by the boiler steam in the latter experiment, and the greater speed of piston with equal piston pressures, a gain not difficult to realize by these means in an engine having the excessive cylinder condensations of that of the *Anthracite*.

The gain due to superheating, *per se*, which is as obtainable from one kind of machinery as another, must be kept separate. It is one thing, and any gain due to high boiler pressures and large measures of expansion is another thing of a totally different kind. The two should not be mingled and confused in comparing the performances of different methods of using steam.

When experiments are made on machinery employing superheated steam the temperature of the steam must be accurately given, otherwise the results are misleading, unintelligible and incapable of correct comparison with those of experiments in which saturated steam, or steam of a lower or higher or lower degree of superheating is employed.

The relative water vaporizing powers of the different coal used in the two experiments may be ascertained from the data corrected for the difference in the conditions. Mr. Bramwell's experiment lasted 11 hours and 10 minutes consecutively; "Nixon's Navigation Coal" was burned with slow combustion, giving only 5 per centum refuse in the form of a loose white ash without clinker, consequently there was no cleaning of the fire requiring the furnace door to be kept open long enough for breaking up and removing clinker; the fire only needed pricking from below the grate. The Engineers' experiment lasted 23 hours and 58 minutes consecutively; semi-bituminous coal of a quality below the average was burned at a higher rate of combustion, giving 17·6363 per centum refuse, one-half of which was clinker in large masses requiring the furnace door to be kept open long enough to break it from the grate and remove it through the door. The results of several very complete experiments on the "Murphy shaking grate, etc.," made in 1878 by a Board of Chief Engineers of the Navy, of which the writer was the presiding officer, showed that the combustible portion of coal underwent an economic loss of 0·3935 per centum for every one per centum the crude coal contained in refuse removed through the furnace door. Accepting this determination, the combustible portion of the coal consumed during the engineers' experiment was reduced in vaporizative power (17·6363 × 0·3935 =) 6·9399 per centum relatively to the coal consumed in Mr. Bramwell's experiment, on which no such reduction is to be made.

The pound of the combustible portion of the coal vaporized, in Mr. Bramwell's experiment, from the temperature of 212 degrees Fahrenheit and under the standard atmospheric pressure, 12·7519 pounds of water.

The pound of the combustible portion of the coal vaporized, in the Engineers' experiment, from the same temperature and under the same pressure, 11·2515 pounds of water, which, corrected for the above 6·9399 per centum due to the refuse, becomes ($\overline{100·0000 - 6·8399}$: 11·2515 : : 100·0000 :) 12·0906 pounds. Consequently the pound of

the combustible portion of "Nixon's navigation coal" was intrinsically

$$\left(\frac{12 \cdot 7519 - 12 \cdot 0906 \times 100}{12 \cdot 0906} = \right) 5 \cdot 47$$ per centum superior in water

vaporizative power to the pound of the combustible portion of the Cumberland semi-bituminous coal. That is to say, if both coals had the same percentage and kind of refuse such would be their relative value in water heating effect. This difference may very easily be allowed.

The very great difference in the water vaporizative power of equal weights of the two crude coals prevents their use for measuring the economic performances of the machinery in the two cases.

THE WEARING POWER OF STEEL RAILS IN RELATION TO THEIR CHEMICAL COMPOSITION AND PHYSICAL PROPERTIES.*

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Read at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17, 1881.

Theo. N. Ely, Esq., Superintendent of Motive Power, Pennsylvania Railroad.

Dear Sir—It is now nearly three years since my first report to you on the subject of steel rails was written. That report, as you will

* At the Lake George meeting of the Institute in October, 1878, I had the honor of presenting to the Institute, through the kind permission of the officers of the Pennsylvania Railroad Company, the results of a study of twenty-five examples of steel rails, which had all been in actual service. Considerable discussion followed the publication of that paper, and there seemed to be a strong disinclination, especially on the part of the steel rail manufacturers, to accept the conclusions presented. One of the principal objections urged against the conclusions drawn was that they were based on too few samples; in other words, that no conclusions safe to act upon could be drawn from the examination of twenty-five rails. In view of this criticism it was decided to repeat the investigation with a larger number of samples, and with the aid of the experience gained in the first investigation. The results of this second study of steel rails are, by the permission of the officers of the Pennsylvania Railroad Company, herewith presented to the Institute, with the sincere desire that they may aid in adding to our knowledge of this most important product. Like the previous paper, this is in the form of a report to one of the officers of the company, which will account for the style, and the manner of presenting the data.

remember, dealt principally with the question of the relation between the chemical composition and physical properties of steel rails and their power to resist crushing and fracture in actual service. Other matters were referred to or touched upon in that report, but the main question was, Why do some rails crush or break in service while others do not? You will doubtless remember that the principal conclusion arrived at was, that the softer rails are less liable to crush or break in service than the hard ones. Or, in other words, so far as conclusions could be drawn from the chemical analysis and physical test of 25 samples of steel rails which had actually been in service, these conclusions were that those rails which have the smaller amounts of carbon, phosphorus, silicon and manganese are less liable to crush or break in service than those which have larger amounts of these elements. Or, again, looked at in the light of physical tests, those rails which have the lower tensile strength and the greater elongation are the ones which give the least trouble from breaking or crushing in track.

In the report just referred to, the question of the wearing power of steel rails was not made prominent, and from that report no positive and definite information could be obtained as to what quality of steel would give rails that would endure the greatest amount of traffic with the least loss of metal. And yet, of the three principal causes which occasion the removal of rails from the track, viz., broken, crushed and worn out, perhaps the latter is of the most importance. With the improvement in maintenance of way which has characterized the Pennsylvania Railroad during the last five or six years, the removal of rails from track from the first two of these causes has, if I am right, quite notably diminshed. This certainly is true with regard to broken rails. And if, as time advances, the number of crushed rails shall diminish, both because of the continued improvement in maintenance of way, before referred to, and because, owing to improved and better methods at the steel works, there are fewer crushed rails caused by physical defects in the steel, the question of the wearing power of steel rails obviously becomes the all-important one. In view of these considerations, it was thought that an investigation into the relation between the wearing power of steel rails and their chemical composition and physical properties could not fail to throw light upon a question of vital importance in the management of the Pennsylvania Railroad. The results of such an investigation are presented in the

following report, which deals not, as did the previous report, with the relations between the chemical and physical characteristics of steel rails and their power to resist crushing or fracture, but entirely with the relation between these characteristics and wear.

On any railroad the rails are or may be called upon to perform their service under quite varying conditions. On a railroad like the Pennsylvania Railroad, for example, there are levels and grades, and there are tangents and curves, and there are combinations of the levels and grades with the tangents and curves. Moreover, the rails on the high sides of curves do their service under different conditions from those on the low sides of curves. So that, as far as kind of service is concerned, a rail may be called upon to do its work under one of these six conditions, viz., on level tangent, on high side of level curve, on low side of level curve, on grade tangent, on high side of grade curve, or on low side of grade curve. Any investigation, therefore, into the wearing power of steel rails, which shall be of service in determining what rails are best for the whole road, must take into account these six conditions. Of course there are many different grades and curves of different radius on the Pennsylvania Railroad; but to study wear on each grade, and for every radius of curvature, would make a problem almost life-long. In order, therefore, to make the work manageable it becomes necessary to choose some average grade and some medium degree of curvature, as representing the different grades and curves on the road. This has been done in the work about to be described.

Now, the problem before us is: What chemical composition and what physical properties are characteristic of those rails which in actual service have lost least metal in proportion to the tonnage that has passed over them? In order to get the information necessary to answer this question, 64 rails were taken from the track in July, 1879, and subjected to chemical analysis and physical test, as is detailed further on. Sixteen of these rails were taken from level tangents and 16 from level curves, 8 from the high side and 8 from the low side of the curves. Again, 16 rails were taken from grade tangents and 16 from grade curves, 8 from the high side and 8 from the low side of these curves. The 32 rails on grades were all taken between Conemaugh and Altoona, and the 32 on levels between Tyrone and Mifflin. Furthermore, 32 of the rails were taken from the north track, and 32 from the south track. The principle governing the selection was to

secure 8 as slow-wearing rails and 8 as rapid-wearing rails as could be found on each of the four conditions of service—level tangents, level curves, grade tangents and grade curves—the rails on curves being taken, as has been already described, one-half from the high side of the curve and one-half from the low side of the curve. In the actual selection a pair of callipers was used, and the loss in height of a rail, compared with its time of service, gave a sufficiently accurate measure of the rate of wear of a rail to determine its selection. It should be stated here that since the rails were selected in July, after the usual annual removal from the track of worn-out rails, which takes place earlier in the season, the more rapid-wearing rails obtained for examination are not as marked examples of rapid-wearing rails as would have been obtained had the selection been earlier made. With regard to the 64 rails chosen, it may also be stated that not one of them had broken in service, and only one showed any signs of crushing; so that, as far as every quality is concerned, except their rate of wear, all of them might be classed as good rails.

It was thought that a chemical examination and physical test of the 64 rails, compared with the loss of metal they had suffered by the tonnage which had passed over them, could not fail to throw some light on the question as to what kind of steel in rails gives best wear. These rails having been selected were removed from the track and sent to Altoona. They were then cleaned and weighed on an ordinary platform scale, their length measured as accurately as possible with a steel tape, and their height callipered near both ends and in the middle. The weight of the whole rail divided by its length obviously gives the present weight per yard of these worn rails. The weights obtained in this manner, however, were not subsequently used, as will be explained further on. After the weighing, five feet were cut off from the end of each rail for test purposes. The test-pieces, except those used in the bending test, were all cut from the head of the rail. Two pieces for tensile test, two for torsion test, and two pieces from the web for bending test, as well as a section of the rail $\frac{1}{2}$ inch thick, were taken from each rail. The tensile test-pieces were 15 inches long, with a reduced section $\frac{3}{4}$ inch in diameter, and 5 inches between shoulders. They also had a groove just beyond each end of the reduced section for holding the micrometer arrangement used in determining the elastic limit. The torsion test-pieces were of the usual size, 4 inches long and 1 inch square, with a reduced section $\frac{5}{8}$ inch in diam-

eter, and 1 inch between shoulders, with a ¼ inch fillet. For the shearing test one-half of the tensile test-pieces, after they were broken, was turned down to a rod ⅝ inch in diameter, and sheared off (single shear) in a shearing apparatus prepared for the purpose. For the bending test two pieces 1½ inches wide, and 12 inches long, were slotted out of the web of each rail. Accompanying Plate 1 gives the details of the form and size of the test-pieces and of the shearing apparatus. A and B represent the shearing apparatus, full size; C represents the tensile test-piece, half size; D, the torsion piece, full size; E, the shearing piece, full size; and F, the bending test-piece, half size. The half-inch section of each rail was used in determining the present weight per yard of the worn rails, the original weight per yard of these rails when laid, as will be described further on, and also in making the diagrams of the worn rails, which appear in the accompanying Plates 2, 3, 4 and 5.

When the tensile test-pieces had been prepared, one-half of them were annealed by heating them to a fair red heat, and then allowing them to cool slowly for 36 hours. Both the annealed and the unannealed test-pieces were then tested in the tensile testing-machine. In determining the elastic limit yokes were fitted to the grooves in the test-pieces prepared for them, which yokes carried micrometer screws on opposite sides of the pieces, reading to the ten thousandth of an inch, and fitted up for electrical contact. The pieces were then strained with successive loads, increasing by 2000 pounds per square inch. After the application of each load the elongation was measured by the micrometers, and the point at which the elongation ceased to be directly proportioned to the load was regarded as the elastic limit. The results of these tests are given on Plates 6 and 7. The figures as to tensile strength and elastic limit mean pounds per square inch. The elongation is per cent. in five inches. Neither of the torsion test-pieces were annealed, they being tested in the condition in which the steel was when removed from the track. The diagrams made by the test of these pieces were measured up, and the results are given in connection with the results of the tensile tests as above described. The figures given as to length of diagram, height of diagram, and elastic limit are in inches and hundredths of an inch; and those giving areas of diagram are square inches. They are the mean of the results from the two test-pieces. In the shearing tests the figures given under "shearing stress" are pounds per square inch; while the "detrusion"

before rupture represents the travel or motion of the shearing piece A, in Plate 1, measured in decimals of an inch. In making these measurements a load of 2000 pounds was put on the pieces and then a reading made; at the moment of rupture another reading was made. The difference of these two readings represents the detrusion. In making the bending tests, the pieces before described were supported on knife edges 10 inches apart, and bent by a third knife edge at equal distances from the supports, after the manner of making transverse tests of metal. In making these tests it was found that the load applied gradually increased until it reached a maximum, and then diminished slowly. The deflection obtained at the point of maximum load varied considerably in the different rails, being a half inch or more each side of two inches. After the maximum load had been obtained as above described, the pieces were removed from the strain, and set up on end in the same machine, and then bent until rupture took place or until they could be bent down no farther. The broken or bent pieces were then laid on a piece of paper in the position in which they were at rupture or as bent, and the amount of deflection from a straight line of the piece broken or bent was measured with a protractor. The figures given under the head "bending tests" are maximum load and deflection. The maximum load was calculated from the data obtained in testing the pieces, for a piece $1\frac{1}{2}$ inches wide and $\frac{1}{2}$ inch thick. The deflection is in degrees. In all cases where the deflection is given as 190°, the pieces did not break, but had the form when removed from the machine of a letter U with the ends brought together until they formed with each other an angle of 10°.

From the torsion test-pieces after they were broken borings were taken for analysis. In these borings the carbon, phosphorus, silicon and manganese were determined. All the chemical work was done in duplicate. The carbon was determined, after separation from the iron by chloride of copper and ammonium, by combustion in oxygen gas, the phosphorus by the molybdate method, the manganese by the bromine method, and the silicon in the usual manner. The result of these determinations are given with the other data. So much for the methods of the chemical and physical testing of these rails.

In determining the rate of wear of a steel rail it is of course necessary to know the loss of metal per yard which each rail has suffered and the tonnage. The loss of metal per yard divided by the tonnage

gives the rate of wear; and when this datum is obtained for a series of rails, it furnishes a means of comparison as to their wearing power.

The tonnage of the 64 rails we are studying was computed from the data as to number of trains and movement of cars in the office of the superintendent of transportation, Mr. John Reilly. These data were twice worked over, and while I do not think that the tonnages given with each rail are absolutely the number of tons which have passed over them, I do think that the percentage error is small, and that the comparison of one rail with another by means of these tonnages, which is really what we are after, gives results that can safely be relied on.

With regard to the loss of metal per yard and the method by which it was obtained it will be necessary to go a little into detail. Of course nothing could be simpler than to obtain the loss of metal per yard of these rails, provided the data were at hand, for this is simply the difference between the present weight per yard of the worn rails and the original weight per yard of these rails when they were laid. But, unfortunately, these rails were not weighed when they were put in track, and so one essential element of our data is wanting. Nor will it do to assume that these rails originally weighed 67 lbs. or 56 lbs. per yard, which is the standard weight of the different rails embraced in this series. Mr. J. W. Cloud, engineer of tests, finds by the weight of a number of new rails just from the mills, weighed at Altoona during two or three years past, that they vary from ½ lb. to 1½ lbs. per yard from standard weight. Still further, an examination of the rails in our series shows that some of them, from the web being thinner than standard, which means that the rolls were closer together when that rail was rolled, could not have weighed when new more than 64 lbs. or 65 lbs. per yard.

In view of these statements, the question fairly meets us: How can the loss of metal per yard of these rails be determined? It is evident to all, I think, that if we know the weight per yard of these worn rails, and then are able to obtain the areas (1) of a section of the worn rail, and (2) of a section of the original rail as rolled, we have at hand all the data necessary for obtaining the weight per yard of the rail as rolled. For it is clear, I am sure, that as the area of a section of the worn rail is to its weight per yard so is the area of a section of the original rail to its weight per yard. If, therefore, we can obtain (1) the weight per yard of the worn rails, (2) the area of a

section of each of these rails, and (3) the area of a section of each rail as rolled, we shall be able to obtain the loss of metal per yard of each of the rails in the series we are studying. Can these data then be obtained?

First, as to the areas. How can the area of a section of the worn rail and of a section of the rail as rolled be obtained? The following was the method used. The half-inch section of the worn rails before referred to was laid upon a sheet of paper, and its outline traced upon the paper with the utmost care by means of a very sharp-pointed hard pencil. The surface inclosed within this tracing corresponds very closely with the section of the worn rail. Then directly over this tracing was laid the template of the section after which this rail was rolled, and a tracing then made with a sharp-pointed hard pencil as before of that portion of the head which was lacking in the worn rail. If now the original height of the rail was the same as the template used, and if the original shape of the head of the rail was that of the template, we have in this manner a diagram on paper representing a section of the worn rail, and also one representing a section of the original rail as rolled. The diagrams obtained in this manner for each rail in the series are represented in accompanying Plates 2, 3, 4 and 5. The areas of these diagrams were then obtained by means of the planimeter.

What now are the assumptions, and what are the probable errors in this method? The first assumption is that the original rails as rolled were the same height as the template. This is probably not exactly true in fact. The wear of the rolls, and possibly carelessness in making the rolls originally, together with the fact that the rails rolled in the same set of rolls were probably not all rolled at the same temperature, and consequently shrunk different amounts in cooling, might each occasion small deviations from the height which the rail should have according to template. And yet Mr. Cloud, engineer of tests, has callipered the height of some 60 new rails here at Altoona during the year past, which rails were from different mills, and has found the variation from standard height not more than $\frac{1}{100}$ of an inch. It would seem, therefore, that the error arising from the assumption as to height of rail could not be large.

The second assumption is that the original shape of the top of the head of the rails we are studying was that of the template. This is of course a question of accuracy in the original manufacture of the

rolls, and of their wear. Now it is impossible to say how accurately the rolls were originally made, and how much they have worn out of shape when any rail was rolled. But in any case the error must be very small, for any noteworthy deviation of the top of the head from standard would have given difficulty in securing a straight, even track, and would have caused the rejection of those rails by the inspector at the mills.

The third assumption is that these rails have not suffered distortion as to height by the service which they have endured. Upon this point I will say that I think an inspection of the diagrams in accompanying Plates 2, 3, 4 and 5 will convince any one that this distortion, if any, is excessively small. It should be stated here that any deviation of these rails from template, such as beading over on one side of the head, or thicker or thinner webs than standard, or variation in the shape of the foot or in the under side of the head, can cause no error; because in taking the areas with the planimeter, the diagrams from which the area of the worn section and the area of the original section were taken, coincide in every part except in that part of the original section which was traced-in from template as above described. In other words, in getting the areas of the rails as rolled, the actual section of the rail just as it was rolled was used in every part, except that portion of the head which was gone from wear, which portion was restored from template.

Fourth. How accurately can the areas be obtained by means of the planimeter? The planimeter which was used gave readings to $\frac{1}{100}$ of a square inch. If, therefore, the manipulation was such as to give the instrument its full chance, the maximum error in area could not be over $\frac{1}{100}$ of a square inch. As a matter of fact, in taking the areas with the planimeter, from three to five measurements were made of each diagram, which measurements differed from each other not more than from one to three hundredths of an inch, and then a mean of these measurements was taken as representing the area of the diagram. It seems probable, therefore, that the error arising from this cause could not in any case amount to more than one or two hundredths of a square inch.

Now as to the weight per yard of the worn rails. How can this best be obtained? Obviously the most simple method would be to weigh each of the worn rails, and then divide the weight by the length. This was done with these rails, as has already been mentioned. But

when the results were obtained, and the original weight of the rails computed, by means of the areas as above described, it was found that some rails rolled at the same mill, at the same time, and with the same thickness of web and same shape of foot, differed from each other in the original weight, as computed, from $1\frac{1}{2}$ to 3 lbs. per yard. The explanation of this seems to be that the half-inch sections before referred to, which were used in getting the areas, did not exactly represent the whole rail; in other words, the rail was unevenly worn. This explanation was confirmed by callipering the height of the half-inch sections, and comparing these heights with the heights of the rail in different places. It became necessary, therefore, to devise some other means of obtaining the weight per yard of the worn rails, and it was finally decided to obtain the desired weights from the half-inch sections themselves before referred to. These sections were all, therefore, carefully weighed on a balance which weighed accurately to one-half a grain. If, now, the half-inch sections were exactly half an inch in thickness, a little consideration shows that it is possible in this way to get at the weight of a rail more accurately than it could be obtained by weighing the whole rail on a common platform scale, supposing, of course, that the rail was uniform in section throughout its length. For there are 72 half-inch sections in a yard, and in a 30-foot rail 720 half-inch sections. If, now, the error in weighing a half-inch section is one-half a grain, the total error in the weight of a 30-foot rail would be 720 half grains, or 360 grains, which is a little less than an ounce. Inasmuch, therefore, as ordinary platform scales do not generally weigh closer than half a pound, it is evident that, so far as accuracy of weighing is concerned, the method of getting at the weight per yard of these worn rails, by weighing the half-inch sections, leaves nothing to be desired.

But were these half-inch sections exactly half an inch thick? An examination of these sections by means of vernier callipers, reading to one-thousandth of an inch, showed that they were not exactly half an inch thick, and that they varied in thickness in different parts of the section. The reasons for this seem to be, that it is almost impossible to set a tool so as to cut off exactly half an inch from a rail, and that in cutting steel as hard as some of these rails were, the tool is apt to spring more or less, and thus give a section of varying thickness. It became necessary, therefore, to devise some method by means of which the average thickness of these sections could be obtained. This was

done as follows: The half-inch sections were all carefully weighed in distilled water, at a temperature of 66° F., on a balance weighing to half a grain. The difference between the weight so obtained and the weight of the same sections in air gives the weight of a volume of water equal to the volume of the section. Now, this weight of water divided by the weight of a cubic inch of water, which was taken as 252·5 grains, gives the volume of space occupied by any section. This volume being known, it is of course only necessary to divide the same by the area obtained by the planimeter, as before described, and the result is the average thickness of each section; and the weight and thickness of each section being known, a very simple calculation gives us the weight per yard, corresponding to each of the worn rails which we are dealing with.

Now, what are the possible errors involved in this method? As to the weight in air of the half-inch sections, it has already been shown that an error of half a grain in the weight of each section gives an error per yard that can safely be ignored. An error of half a grain in weighing the sections in water gives an error in the weight per yard of the worn rail of ·05 of a pound; and an error of a hundredth of a square inch in taking the areas of the sections with the planimeter causes an error in the weights of about a tenth of a pound. It seems fair, therefore, to conclude that probably the weights of the worn rails and of these rails when originally laid, as obtained in the manner above described, do not differ from the true weights more than possibly a quarter of a pound per yard.

It is, of course, to be confessed that whatever errors there may be, either in the weight of the worn rails or in the original weight of these same rails, appears in the loss of metal due to wear—which is really what we are after—and which is simply the difference between these two weights. But it must be remembered that, in determining the value of a rail, this loss of metal is subsequently divided by the tonnage, and consequently the influence of the error is thereby very greatly diminished. And while there may be a few rails in the series which occupy a place which they would not occupy if the loss of metal were more accurately known, yet I think these cases are too few to seriously obscure or counteract the general conclusions which the results that have been obtained are calculated to teach.

In the following pages are given the history of each rail and the tonnage. The essential parts of this history, together with the ton-

nage, the chemical analyses, the weights per yard, and loss of metal, as well as the loss per million tons, and the complete results of the physical tests and the density, are given in tabular form in accompanying Plates 6 and 7. These tables will be discussed farther on. The density is the weight of a cubic inch of the steel. This weight was obtained by dividing the weight of each of the half-inch sections by its volume, the data for this purpose having been obtained as previously described. The figures given under density are fractions of a pound.

In looking over the original weights of the rails as obtained, it will doubtless be observed that, although these rails were all supposed to have weighed 67 lbs. or 56 lbs. per yard when rolled, the figures given differ both ways from these figures. These differences are principally to be accounted for by differences in the shape of the bases and differences in the thickness of the webs. With regard to the latter point, it is evident that of two rails rolled at the same mill, in the same year, if one has a web three or five one-hundredths of an inch thicker than another—which means that the rolls were farther apart when the thick-web rail was rolled—the thin-web rail will be lighter than the other. The thickness of the webs of all the rails in the series we are studying was carefully measured with vernier callipers, and the differences from the standard thickness were found to vary both ways, from nothing up to five one-hundreths of an inch thinner than standard, and seven one-hundredths thicker than standard. Finally, the differences in the density of the different rails still further helps us to account for the differences in weight per yard of the rails as rolled.

The following is the history and tonnage of the different rails:

No. 881. Steel of 1868. Was on tangent, north rail, north track, in Bennington Cut. In service from June, 1868, to July, 1879—11 yrs. 1 mo. Grade, 92·4 ft. to the mile. Tonnage, 55,546,811 tons.

No. 882. Steel of 1868. Was on tangent, north rail, north track, in Bennington Cut. In service from June, 1868, to July, 1879—11 yrs. 1 mo. Grade, 92·4 ft. to the mile. Tonnage, 55,546,811 tons.

No. 883. Steel of 1869. Was on tangent, north rail, north track, on Whip-poor-will Straight. In service from May, 1869, to July,

1879—10 yrs. 2 mos. Grade, 95·04 ft. to the mile. Tonnage, 52,174,969 tons.

No. 884. Steel of 1869. Was on tangent, north rail, north track, on Whip-poor-will Straight. In service from May, 1869, to July, 1879—10 yrs. 2 mos. Grade, 95·04 ft. to the mile. Tonnage, 52,174,969 tons.

No. 885. Steel of 1868. Was on tangent, south rail, north track, just west of Allegrippus Station. In service from July, 1868, to July, 1879—11 yrs. Grade, 89·76 ft. to the mile. Tonnage, 55,197,994 tons.

No. 886. Steel of 1868. Was on tangent, north rail, north track, just west of Allegrippus Station. In service from July, 1868, to July, 1879—11 yrs. Grade, 89·76 ft. to the mile. Tonnage, 55,197,994 tons.

No. 887. Steel of 1868. Was on tangent, south rail, north track, west of Allegrippus Station. In service from July, 1868, to July, 1879—11 years. Grade, 89·76 ft. to the mile. Tonnage, 55,197,994 tons.

No. 888. Steel of 1868. Was on tangent, south rail, north track, west of Allegrippus Station. In service from July, 1868, to July, 1879—11 years. Grade, 89·76 ft. to the mile. Tonnage, 55,197,994 tons.

No. 889. Steel of 1873. Was on tangent, south rail, south track, at South Fork. In service from August, 1873, to July, 1879—5 yrs. 11 mos. Grade, 21·13 ft. to the mile. Tonnage 44,620,100 tons.

No. 890. Steel of 1873. Was on tangent, north rail, south track, at South Fork. In service from August, 1873, to July, 1879—5 yrs. 11 mos. Grade, 21·13 ft. to the mile. Tonnage, 44,620,100 tons.

No. 891. Steel of 1872. Was on tangent, south rail, south track, at Summer Hill water plug. In service from June, 1872, to July, 1879—7 yrs. 1 mo. Grade, 40·13 ft. to the mile. Tonnage, 53,687,-192 tons

No. 892. Steel of 1872. Was on tangent, south rail, south track, at Summer Hill water plug. In service from June, 1872, to July, 1879—7 yrs. 1 mo. Grade, 40·13 ft. to the mile. Tonnage, 53,687,-192 tons.

No. 893. Steel of 1874. Was on tangent, north rail, south track,

near Summer Hill. In service from May, 1874, to July, 1879—5 yrs. 2 mos. Grade, 21·12 ft. to the mile. Tonnage, 38,088,572 tons.

No. 894. Steel of 1874. Was on tangent, south rail, south track, east of Portage. In service from May, 1874, to July, 1879—5 yrs. 2 mos. Grade, 52·8 ft. to the mile. Tonnage, 38,088,572 tons.

No. 895. Steel of 1874. Was on tangent, south rail, south track, east of Portage. In service from May, 1874, to July, 1879—5 yrs. 2 mos. Grade, 52·8 ft. to the mile. Tonnage, 38,088,572 tons.

No. 896. Steel of 1874. Was on tangent, south rail, south track, east of Portage. In service from May, 1874, to July, 1879—5 yrs. 2 mos. Grade, 52·8 ft. to the mile. Tonnage, 38,088,572 tons.

No. 897. Steel of 1868. Was on high side of 5° curve, north rail, north track, east of Bridge No. 3, Summer Hill. In service from May, 1868, to July, 1879,—11 yrs. 2 mos. Grade, 21·12 ft. to the mile. Tonnage, 52,370,617 tons.

No. 898. Steel of 1868. Was on high side of 5° curve, north rail, north track, east of Bridge No. 3, Summer Hill. In service from May, 1868, to July, 1879—11 yrs. 2 mos. Grade, 21·12 ft. to the mile. Tonnage, 52,370,617 tons.

No. 899. Steel of 1868. Was on low side of 5° curve, south rail, north track, east of bridge No. 3, Summer Hill. In service from May, 1868, to July, 1879—11 yrs. 2 mos. Grade, 21·12 ft. to the mile. Tonnage, 52,370,617 tons.

No. 900. Steel of 1868. Was on low side of 5° curve, north rail, north track, east of Bridge No. 3, Summer Hill. In service from May, 1868, to July, 1879—11 yrs. 2 mos. Grade, 21·12 ft. to the mile. Tonnage, 52,370,617 tons.

No. 901. Steel of 1871. Was on low side of 5° curve, south rail, north track, in Gable's Cut, near Summer Hill. In service from August, 1871, to July, 1879—7 yrs. 11 mos. Grade, 39·6 ft. to the mile. Tonnage, 40,061,230 tons.

No. 902. Steel of 1871. Was on high side of 5° curve, north rail, north track, in Gable's Cut, near Summer Hill. In service from August. 1871, to July, 1879—7 yrs. 11 mos. Grade, 39·6 ft. to the mile. Tonnage, 40,061,230 tons.

No. 903. Steel of 1876. Was on high side of 5° curve, south rail, south track, west of Bridge No. 1, Summer Hill. In service from

August, 1876, to July, 1879—2 yrs. 11 mos. Grade, 39·6 ft. to the mile. Tonnage, 21,504,824 tons.

No. 904. Steel of 1876. Was on low side of 5° curve, north rail, south track, west of bridge No. 1, near Summer Hill. In service from August, 1876, to July, 1879—2 yrs. 11 mos. Grade, 39·6 ft. to the mile. Tonnage, 21,504,824 tons.

No. 905. Steel of 1868. Was on high side of 5° curve, north rail, north track, east end of Milmore Middle Siding. In service from June, 1868, to April, 1879—10 yrs. 10 mos. Grade, 47·52 ft. to the mile. Tonnage, 50,648,939 tons.

No. 906. Steel of 1868. Was on low side of 5° curve, south rail, north track, east end of Wilmore Middle Siding. In service from June, 1868, to April, 1879—10 yrs. 10 mos. Grade, 47·52 ft. to the mile. Tonnage, 50,648,939 tons.

No. 907. Steel of 1874. Was on high side of 5° curve, north rail, north track, east of Wilmore Middle Siding. In service from June, 1874, to April, 1879—4 yrs. 10 mos. Grade, 47·52 ft. to the mile. Tonnage, 24,211,147 tons.

No. 908. Steel of 1875. Was on low side of 4° curve, north rail, south track, near tower at east end of Galitzin Tunnel. In service from May, 1875, to July, 1879—4 yrs. 2 mos. Grade, 95·04 feet to the mile. Tonnage, 32,428,614 tons.

No. 909. Steel of 1875. Was on high side of 4° curve, south rail, south track, near tower at east end of Galitzin Tunnel. In service from May, 1875, to July, 1879—4 yrs. 2 mos. Grade, 95·04 ft. to the mile. Tonnage, 32,428,614 tons.

No. 910. Steel of 1871. Was on low side of 5° curve, south rail, south track, west of Allegrippus Station. In service from July, 1871, to July, 1879—8 yrs. Grade, 89·76 ft. to the mile. Tonnage, 62,813,664 tons.

No. 911. Steel of 1877. Was on low side of 5° curve, south rail, south track, second curve west of Allegrippus Tower. In service from April, 1877, to July, 1879—2 yrs. 3 mos. Grade, 89·76 ft. to the mile. Tonnage, 17,226,993 tons.

No. 912. Steel of 1873. Was on high side of 5° curve, north rail, south track, second curve west of Allegrippus Tower. In service from July, 1873, to July, 1879—6 yrs. Grade, 89·76 ft. to the mile. Tonnage, 47,438,145 tons.

No. 913. Steel of 1870. Was on tangent, north rail, north track,

600 feet west of mile post 211 from Philadelphia. In service from March, 1870, to July, 1879—9 yrs. 4 mos. On level. Tonnage, 45,855,101 tons.

No. 914. Steel of 1870. Was on tangent, south rail, north track, 600 feet west of mile post 211 from Philadelphia. In service from March, 1870, to July, 1879—9 yrs. 4 mos. On level. Tonnage, 45,855,101 tons.

No. 915. Steel of 1873. Was on tangent, south rail, north track, east of Petersburg Toolhouse. In service from June, 1873, to July, 1879—6 yrs. 1 mo. On level. Tonnage, 31,514,889 tons.

No. 916. Steel of 1873. Was on tangent, north rail, north track, just west of Ardenheim. In service from July, 1873, to July, 1879—6 yrs. On level. Tonnage, 31,127,829 tons.

No. 917. Steel of 1873. Was on tangent, south rail, north track, just west of Ardenheim. In service from July, 1873, to July, 1879—6 yrs. On level. Tonnage, 31,127,829 tons.

No. 918. Steel of 1869. Was on tangent, north rail, north track, west of Vandevaner's Bridge. In service from January, 1869, to July, 1879—10 yrs. 6 mos. On level. Tonnage, 51,720,011 tons.

No. 919. Steel of 1869. Was on tangent, north rail, north track, west of Vandevaner's Bridge. In service from January, 1869, to July, 1879—10 yrs. 6 mos. On level. Tonnage, 51,720,011 tons.

No. 920. Steel of 1867. Was on tangent, south rail, north track, at Jackstown Water-trough. In service from December, 1867, to July, 1879—11 yrs. 7 mos. On level. Tonnage, 52,991,684 tons.

No. 921. Steel of 1874. Was on tangent, north rail, north track, 200 feet east of mile post 211 from Philadelphia. In service from March, 1874, to July, 1879—5 yrs. 4 mos. On level. Tonnage, 27,622,230 tons.

No. 922. Steel of 1874. Was on tangent, south rail, north track, 200 ft. east of mile post 211 from Philadelphia. In service from March, 1874, to July, 1879—5 yrs. 4 mos. On level. Tonnage, 27,622,230 tons.

No. 923. Steel of 1873. Was on tangent, south rail, north track, east of Petersburg Toolhouse. In service from June, 1873, to July, 1879—6 yrs. 1 mo. On level. Tonnage, 31,514,889 tons.

No. 924. Steel of 1875. Was on tangent, south rail, south track, west of Vandevaner's Bridge. In service from June, 1875, to July, 1879—4 yrs. 1 mo. On level. Tonnage, 36,349,989 tons.

No. 925. Steel of 1875. Was on tangent, south rail, south track, west of Vandevaner's Bridge. In service from June, 1875, to July, 1879—4 yrs. 1 mo. On level. Tonnage, 36,349,989 tons.

No. 926. Steel of 1870. Was on tangent, south rail, south track, 1500 feet west of mile post 182 from Philadelphia. In service from April, 1870, to July, 1879—9 yrs. 3 mos. On level. Tonnage, 76,409,123 tons.

No. 927. Steel of 1870. Was on tangent, south rail, south track, 1500 feet west of mile post 182 from Philadelphia. In service from April, 1870, to July, 1879—9 yrs. 3 mos. On level. Tonnage, 76,409,123 tons.

No. 928. Steel of 1874. Was on tangent, south rail, south track, 200 feet east of mile post 181 from Philadelphia. In service from July, 1874, to July, 1879—5 yrs. On level. Tonnage, 43,610,150 tons.

No. 929. Steel of 1867. Was on high side of $2\frac{1}{4}°$ curve, north rail, north track, 250 feet east of mile post 194 from Philadelphia. In service from December, 1867, to July, 1879—11 yrs. 7 mos. On level. Tonnage, 52,991,684 tons.

No. 930. Steel of 1867. Was on low side of $2\frac{1}{4}°$ curve, south rail, north track, 250 feet east of mile post 194 from Philadelphia. In service from December, 1867, to July, 1879—11 yrs. 7 mos. On level. Tonnage, 52,991,684 tons.

No. 931. Steel of 1868. Was on high side of 3° curve, south rail, north track, 400 feet east of mile post 191 from Philadelphia. In service from January, 1869, to July, 1879—10 yrs. 6 mos. On level. Tonnage, 51,720,011 tons.

No. 932. Steel of 1868. Was on low side of 3° curve, north rail, north track, 400 feet east of mile post 191 from Philadelphia. In service from January, 1869, to July, 1879—10 yrs. 6 mos. On level. Tonnage, 51,720,011 tons.

No. 933. Steel of 1869. Was on high side of $3\frac{1}{2}°$ curve, north rail, south track, 1000 feet east of mile post 183 from Philadelphia. In service from November, 1869, to July, 1879—9 yrs. 8 mos. On level. Tonnage, 78,364,968 tons.

No. 934. Steel of 1869. Was on low side of $3\frac{1}{2}°$ curve, south rail, south track, 1000 feet east of mile post 183 from Philadelphia. In service from November, 1869, to July, 1879—9 yrs. 8 mos. On level. Tonnage, 78,364,968 tons.

No. 935. Steel of 1866. Was on high side of 2° curve, north rail, south track, at McVeytown Station. In service from September, 1866, to June, 1879—12 yrs. 9 mos. On level. Tonnage, 92,025,-478 tons.

No. 936. Steel of 1866. Was on low side of 2° curve, south rail, south track, at McVeytown Station. In service from September, 1866, to June. 1879—12 yrs. 9 mos. On level. Tonnage, 92,025,478 ton

No. 937. Steel of 1876. Was on low side of 2° curve, south rail, south track, 200 feet west of mile post 192 from Philadelphia. In service from March, 1876, to July, 1879—3 yrs. 4 mos. On level. Tonnage, 29,905,122 tons.

No. 938. Steel of 1876. Was on high side of 2° curve, north rail, south track, 200 feet west of mile post 192 from Philadelphia. In service from March, 1876, to July, 1879—3 yrs. 4 mos. On level. Tonnage, 29,905,122 tons.

No. 939. Steel of 1875. Was on the low side of 4° curve, south rail, south track, 250 feet east of Mount Union Bridge. In service from May, 1875, to July, 1879—4 yrs. 2 mos. On level. Tonnage, 37,150,179 tons.

No. 940. Steel of 1875. Was on the high side of 4° curve, north rail, south track, 250 feet east of Mount Union Bridge. In service from May. 1875, to July, 1879—4 yrs. 2 mos. On level. Tonnage, 37,150,179 tons.

No. 941. Steel of 1874. Was on high side of 3° curve, south rail, south track, 300 feet east of Manayunk. In service from September, 1874 to July. 1879—4 yrs. 10 mos. On level. Tonnage, 42,277,638 tons.

No. 942. Steel of 1874. Was on low side of 3° curve, north rail, south track, 300 feet east of Manayunk. In service from September, 1874, to July. 1879—4 yrs. 10 mos. On level. Tonnage, 42,277,638 tons.

No. 943. Steel of 1873. Was on the low side of 2° curve, south rail, south track, 2000 feet west of mile post 179 from Philadelphia. In service from April, 1873, to July, 1879—6 yrs. 3 mos. On level. Tonnage, 55,127,464 tons.

No. 944. Steel of 1873. Was on high side of 2° curve, north rail. south track, 2000 feet west of mile post 179 from Philadelphia. In

service from April, 1873, to July, 1879—6 yrs. 3 mos. On level. Tonnage, 55,127,464 tons.

Accompanying Plates 6 and 7 give in tabular form all the essential data of the history of the rails, together with the chemical analyses, weights, and results of the chemical tests, as has been before stated. To a study of these tables attention is now directed. As will be observed, the rails have been arranged in six groups, according to the kind of service to which they have been subjected. The first group of 16 rails did their service on grade tangents; the second group of 8 rails on the low side of grade curves; the third group of 8 rails on the high side of grade curves; the fourth group of 16 rails on level tangents; the fifth group of 8 rails on the low side of level curves; and the sixth group of 8 rails on the high side of level curves. As will likewise be observed, the rails within the groups have been arranged in regular order according to loss of metal per million tons, the rail having lost least metal per million tons being placed first. The first rails in the several groups are therefore the slower wearing, while the latter rails are the more rapid wearing. This arrangement enables us to compare the slower-wearing rails of the different kinds of service with the more rapid-wearing ones, and to discover what physical qualities, and what chemical composition, are characteristic of the slower-wearing as well as the faster-wearing rails.

Giving our attention now to the tables, I think the first observation will be that there is no absolute gradation in physical qualities, or in chemical composition, applying to every rail in each group, which corresponds to the gradation in amount of metal lost per million tons. In other words, if we take tensile strength, or elongation in the physical qualities, or the carbon or phosphorus in the chemical composition, we do not find that the first rail in each group is characterized by a certain figure in any one or more of these respects, while the last rail in the group is characterized by another different figure, and all the intermediate rails arrange themselves in respect to any of these peculiarities, between these two extreme rails of the group. Nor do I think we ought to expect such uniformity, and for the following reasons:

First. Whatever errors there may have been in determining the loss of metal of these rails or in the tonnages, as has been before described, will of course have an influence in determining the position

of any rail in the group to which it belongs. And, as has already been said, while I do not think that these errors, whatever they may be, are sufficient to seriously obscure or counteract the general conclusions which the results are calculated to teach, yet these errors may possibly be large enough to give some rail a position in the group which it should not occupy.

Second. I am not aware that it is known as yet exactly what *wear* is, or what it is dependent upon. Some considerations in regard to the nature of wear will be advanced further on; but whether wear is a direct function of the tensile strength of steel, or of its elongation, or of its elastic limit, or of its resilience, or any combination of these, or indeed, as seems somewhat probable, of the amount of distortion by bending that a piece of steel will suffer, is a problem yet to be solved. It may be that wear bears a direct ratio to the elongation within the elastic limit, or to the amount of work done within the elastic limit whenever a particle is worn off; or, indeed, wear may be a direct function of the granular structure of steel, the wear being more rapid as the granular structure is coarser, or *vice versa*. Or, finally, wear may be due to a combinations of causes, and each cause may require for its elucidation a different physical test. It is perhaps, therefore, not strange, in view of this want of knowledge, that, in a series of steels arranged according to loss of metal, or on the principle of wear, the physical qualities which we are now able to measure should not show uniform gradations throughout the series. But, it seems to me, this reasoning does not tend to throw doubt on our ability to draw conclusions that will be valuable from the work which we have in hand. For the question we are studying is not what does wear depend upon, but what chemical composition, and what physical properties, are, in general, characteristic of such rails as have in actual service given least loss of metal per million tons' burden? While the answer to this question may not solve the whole problem of wear, yet it cannot fail to throw light on the relation between the chemistry and physics of steel and its wearing power, which, after all, with our present knowledge of steel metallurgy, is all the knowledge we are able to utilize.

(To be continued.)

NOTE ON STEAM CYLINDERS.

By WILLIAM DENNIS MARKS,
Whitney Professor of Dynamical Engineering, University of Pennsylvania.

In the majority of steam engines hitherto built but little thought seems to have been given to the relative lengths of the stroke and diameter of the steam cylinder, as affecting the wasteful condensation of the steam used.

The writer has already called attention to the fact that, if the condensation is solely dependent upon the surface exposed to the steam, as compared with the cylinder's volume, that the steam cylinder should have an equal stroke and diameter.*

On the other hand, it should also be borne in mind that the time of exposure of any condensing surface may affect the amount of condensation, and that if we regard the amount of condensation as directly proportional to the time of the exposure of any given area of condensing surface, *the amount of condensation becomes proportional to the product of the surface exposed by its time of exposure.*

Since, however, the interior of a cylinder continually becomes hotter, approaching more nearly to the temperature of the steam the longer it is exposed, we see that the condensation is not directly proportional to the time of exposure of any unit of area, and it seems natural to suppose that the condensation at first contact with the colder sides of the cylinder would deposit a dew, or film of moisture, and that the known slow powers of conduction of iron for heat would prevent much additional condensation in the time usually allotted to one stroke, thus rendering the amount of condensation almost independent of the time of exposure of the condensing surface.

If, however, we do assume that the wasteful condensation is proportional to the time of exposure as well as the area of the surface exposed, it can be shown, by means of the differential calculus, that the stroke of any steam cylinder should equal twice its diameter, in order that the condensation shall be a minimum for any given volume of cylinder.

* The Relative Proportions of the Steam Engine. Lecture I. Marks. Lippincott & Co. 1879.

Cotterill, in "The Steam Engine," advances the theory that the water of condensation of the steam, while expanding and doing useful work, is carried out in the exhaust, and that none of it remains in the cylinder to interfere with the entering steam at the next stroke, and further, that the water wastefully condensed at the high temperature of the entering steam is re-evaporated, at least in part, if the engine is worked expansively during the latter part of the stroke, when the pressure and temperature are less, heat being given out by the cylinder. Thus we see that if time is assumed to have no effect upon the amount of condensation, we have stroke and diameter equal; if the condensation is proportional to the time, the stroke equals twice the diameter; and further, the condensation is greater as the difference of temperatures of the cylinder walls, and the steam is greater, and the time it is allowed to act is increased, which seems to demand superheating of the steam in order to have enough surplus heat to heat up the cylinder without wasteful condensation, and a diminution of the surface to be heated up with regard to the time of contact. Much speculation has been indulged in on these points, but we have as yet no experiments which will do more than verify theories in the rudest manner.

We are safe in assuming, until we have much more accurate experimental data than at present giving proof to the contrary, that the time proportion of stroke to diameter in steam cylinders lies much nearer one than two.

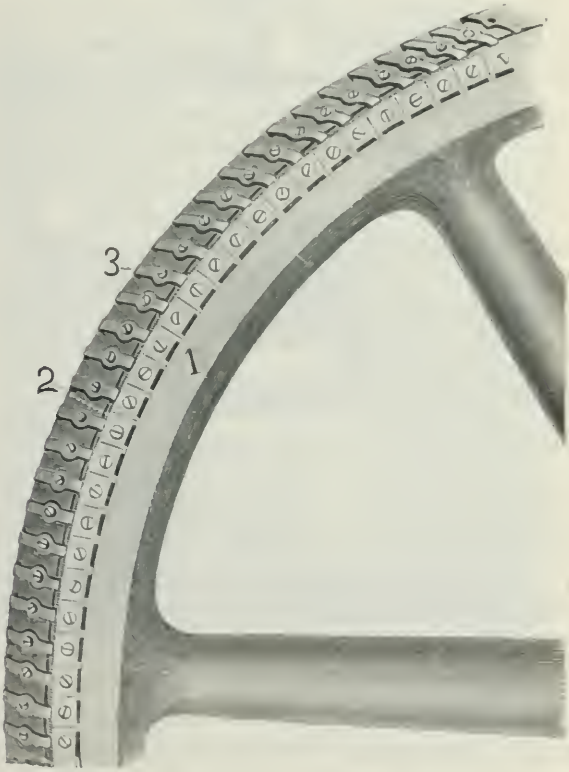
Philadelphia, January, 1881.

Cheap Motors.—A prize of 1000 fr. ($200) has been awarded to M. de Bisschop for a small motor, suited to family use, on the recommendation of M. Tresca, as chairman of the committee. Although it is worked by gas, which is always a dear fuel, this inconvenience is more than compensated by the economy attendant upon the small amount of care that is required. Machines which do a work of 5 kilogrammetres (36·17 foot pounds) per second, use only two cents' worth of gas per hour, at the Paris price; the machine of 25 kilogrammetres (180·8 foot pounds) use five cents' worth. The larger machines are sold for $180, the smaller for $100.—*Bull. de la Soc. d'Encour.* C.

NOVEL MODE OF ORIGINATING AN INDEX WHEEL.

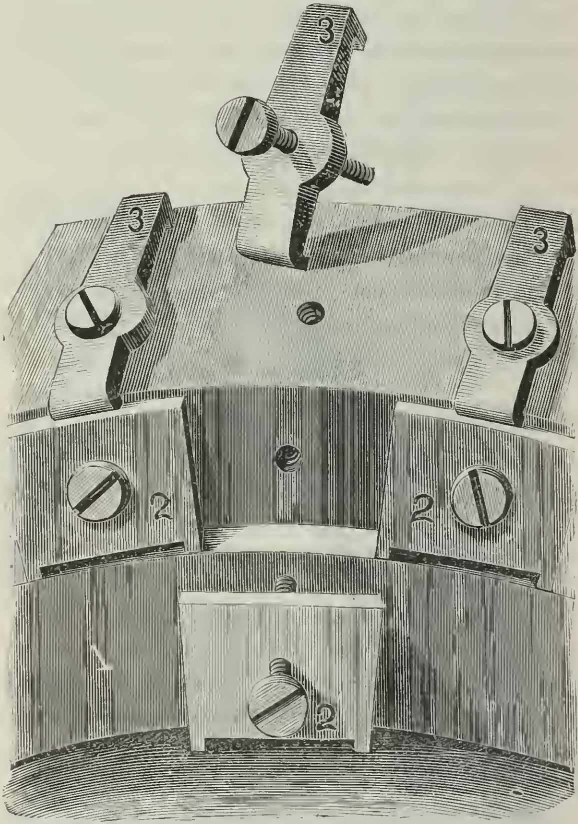
By Robert Grimshaw, Ph.D.

In the manufacture of printing presses more absolute accuracy is required in the main spur wheels and the racks which drive the bed than in almost any other kind of machinery, if indeed, the demands of any other class be so exacting. The Messrs. Hoe & Co., of New York, not satisfied with results which, while satisfying their patrons, might at the same time fall short of the requirements of advanced design and construction, brought forth by keen competition, resolved to obtain an original dividing wheel which would ensure absolute timing in the presentation of all spur wheels cut from it.



One specially made by Whitworth was returned as not equal to what the Hoes had themselves accomplished, and it was resolved to make a new one "for all time."

An attempt was made to graduate a 20 feet wheel, of seasoned wood, on a vertical axis, kept in a cellar with cement floor. But temperature and moisture interfered with this, and a 6 feet wheel was made of cast iron, and the divisions determined, not by dividing, but by building up. One hundred and eighty pieces of cast iron were filed true to gauge and built around the accurately turned rim, each being held by a screw and a clamp.



One of these pieces being removed serves as a stop, to cut one tooth of the main spur wheel of the gear cutter; and then the opposite tooth is cut, and so on, every precaution being taken, as slow cutting, cutting alternately on opposite sides of the rim, to guard against errors from temperature.

The one hundred and eighty pieces were of cast iron, were exactly alike and interchangeable.

Great care was taken in fitting them to keep the metal cool, as it was found that holding one of the pieces, which had been fitted to gauge, in the hand for a few seconds would expand the metal so that it would not reach the bottom of gauge by a sixteenth of an inch: also, that inserting a piece of tissue paper, equal in thickness to the thousandth part of an inch, in the side of gauge would have the same effect. When the pieces were first fitted to the wheel it was found that they lacked three-sixteenth inch of a perfect circle, which made it necessary to reduce very slightly the diameter of the wheel. This was done by revolving the wheel very slowly by hand on a vertical fixed axis, which accurately fitted the hole in wheel, against a stationary cutter, very hard and very sharp, making necessarily more of a scrape than a cut. Then the pieces were again fitted, and it was found all had to be altered, owing to the change in the size of wheel. This operation was repeated many times, and although much time was spent it was deemed advisable in attaining the perfection desired.

THE POLARIZATION OF SOUND AND THE NATURE OF VIBRATIONS IN EXTENDED MEDIA.*

By S. W. Robinson,

Professor of Physics and Mechanics, Ohio State University.

The phenomena of polarization of light, heretofore supposed due to transversal vibrations can be explained on the basis of longitudinal vibrations alone. Polarization of sound, extends the theory of longitudinal vibrations to those in all possible substantial media, including luminous ones.

The object of the following article is to show, by theory and experiment, that longitudinal vibrations, such as in sound waves, can be polarized; and not only this, but also to show that it is irrational and improbable for vibrations in extended media generally to be primarily otherwise than longitudinal. All this is aimed especially at the *transversal theory* of light.

It is well known that light can be radiated, reflected, refracted,

diffracted, diffused, can be made to interfere and can be polarized. All these effects are known to be common to sound, except the last; and it is for the sole purpose of explaining this in light that the convenient theory of transversal vibration has been set up by physicists, for the single case of luminous vibration. It is, therefore, only necessary to polarize sound to place all the known effects of luminous waves in common with sound waves, or to make the theory of longitudinal vibrations universal.

Assent to the above notions will be the more readily given after noticing the consideration that, in polarized light it is not necessary to suppose the vibrations transversal till after passing the polarizer, and that the latter imparts an effect equivalent to a lateral impulse, as due to its one-sided action upon the ray transmitted, thus giving cause for vibrations which are more or less transversal; also it is to be noticed that transversal vibrations are not to be assumed impossible when sufficient cause exists; it is simply assumed that the cause is insufficient when a material particle is made to vibrate from the action of a disturbance at a remote centre transmitted to the particle considered the centre, the transmission, and the particle considered being supposed as belonging to a homogeneous medium of indefinite extent. As regards the nature of the vibratory movements of particles of luminiferous ether may we not justly ask that, if we can go through such a range of density as from platinum to hydrogen without a change in the nature of the vibrations, where, as we rise in the scale of ethereal tenuity, shall longitudinal end and transversal begin? Why should the luminiferous ether, now considered as a substance, have a peculiar form of vibration? If ether undulations can be polarized, why not undulations generally? These questions are not answered by the highest authorities. The short of it all seems to be that, if polarized light had never been discovered probably the device of transversal vibrations never would have been set up. Indeed, so eminent an author as M. J. Jamin says in his three volume work on Physics at the outset, in his lesson on polarization, and subsequent to the treatment of interference, diffraction and other phenomena: "What has been said previously of the movement of luminous waves is absolutely independent of the directions of the vibration. * * * We have seen in acoustics that the oscillations of the molecules of air are in the directions of propagation of sound; the same when rods or strings vibrate longitudinally, but are perpendicular to the direction

of propagation in waves on a liquid, or when strings and rods vibrate transversely. In general, imagine in any medium any centre of disturbance whatever; it imparts to any given molecule a vibratory movement, generally oblique to the direction of radiation, and which can be decomposed into two others, one longitudinal and the other transversal. Experiment and calculation prove that these vibrations give rise to two species of concentric waves, which will have unequal velocities of propagation. * * * These considerations apply to the ether as well as to heavy bodies. We ask if these waves coexist in this ether, which produces the light better, the longitudinal or the transversal vibrations? This question can only be answered by experiment, which we now proceed to do."

This is a more complete statement than usually met with on undulations. We find, indeed, a denial of any necessity for transversal vibrations except in polarization, and even there a general question is raised as to the direction of vibration, but evidently with the expectation of deciding it in favor of transversal by experimental polarization. Also, we find support of the doctrine of longitudinal vibrations for media of indefinite extent, and of transversal at the limiting surfaces of media, such as in waves on water. But the general theory of oblique vibrations resolved to components we find beset with doubts and difficulties, especially if we are to admit unequal velocities of propagation, as claimed. For instance, what particular component is the velocity of light to have? Do the varieties in velocity account for the varieties in color? Even this would compass but a part of the components, and what office have the remaining ones? A peculiar obliquity, or component of the same, would necessitate a peculiar condition of the radiant proper for provoking a certain obliquity, etc., in the surrounding ether. If this be granted, what is the character of a vibration at a distance from the radiant? Suppose a radiant exciting oblique vibrations in particles of the ether near it; this movement, transmitted to the next particle, will tend to be in a parallel line, and so on from particle to particle; likewise for another vibration at another point of surface of radiant. This would cause all lines of vibratory motion to have such directions as to pass through the radiant, so that at a remote point all particles would be found vibrating in lines contained within a visual angle of the radiant, with respect to that point. For example, in direct sunlight all lines of vibration of particles at the earth would lie within the angle of the sun, of about

half a degree, and hence almost perfectly longitudinal. Again, as to the motion of a remote particle, it should be the resultant of all actions extended to it from the radiant, and hence longitudinal. In support of this we have the famous principle of Helmholtz regarding the action of natural forces among mutually interacting material points, viz.: that the forces must be central forces and functions of the distance, and hence motions of remote particles can only be longitudinal with reference to the centre of force.

From these considerations transversal vibrations, at a considerable distance from a radiant, seem impossible, hence if light can be polarized why not undulations generally. The writer, after much study, became convinced of the possibility of this about eight years ago. At two different periods apparatus was made for putting the matter to an experimental test, and this was done last May with satisfactory results. I propose now to describe the apparatus and give the results.

The means adopted for polarizing the undulations is the same as that for polarizing light by reflection. It is well known that when sound passes from one medium into another whose velocity of sound differs, the sound is refracted. Recent investigations of Henry, Tyndall and others have indicated that when sound encounters a change of density of medium, as when passing from clear atmosphere into a wall of fog, there is a reflection of sound. Altogether there seems no doubt but sound acts like light in these respects, that is, on meeting a change of refractive power, it is both reflected and refracted, as light is at the surface of water or of glass. The reflected light being polarized, the reflected sound is supposed to be.

Applying the laws of Fresnel and Brewster—1st, that the index of refraction is equal to the ratio of the velocities of the waves in the media; and 2d, that complete polarization is obtained for the particular case of right angled reflected and refracted component rays, we are guided to the proper conditions. We conclude that any two substances having different velocities of propagation of waves may be selected. For instance, two gases, like hydrogen and air, any two liquids, any two solids, a solid and a gas, or, generally, any two media whatever. Considerations of convenience would indicate air and illuminating gas, and these were chosen for the present purpose. The velocities of propagation in air and coal gas being as 1125 and 1420, the index of refraction, according to the first law above, is $n = 1{\cdot}26$.

The second law gives for the polarizing angle of incidence, tanget $i = n = 1{\cdot}26$, or $i = 51\frac{3}{4}°$, the rays or waves being in the gas. To realize this incidence upon a surface of separation between the gas and air, the cool gas was placed in L-shaped tubes, *AB*, Fig. 1, having a portion cut away at the angle, as shown at *CD*. The branches of the L

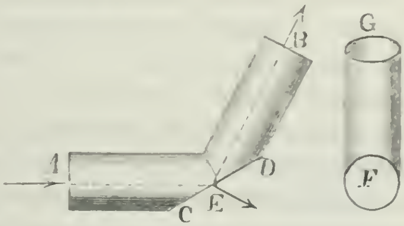


Fig. 1.

make equal angles of $51\frac{3}{4}°$ with the normal to *CD*. A delicate membrane was gummed to the tube covering the opening at *CD*, also shown at *F*, the object of which was to retain the gas and maintain a polarizing surface, *CD*. The arrow at *A* indicates a ray which is incident at *E*, and is then in part refracted outward at *E* in a direction perpendicular to the reflected component *EB*. Each tube was about one inch in diameter and three inches long. A number of these were made of tin, each with one end slightly larger than the other, so that they could be joined up, stove-pipe fashion, to any desired extent. Being cylindrical, the plane of one L piece could be placed at any angle with the plane of the preceding one, according to the desired polarizing test.

Fig. 2 shows the manner of joining the tubes, giving the effect of nine polarizing surfaces, like nine plates of glass in light arranged at the polarizing angle. The nine plates of glass can be used in two

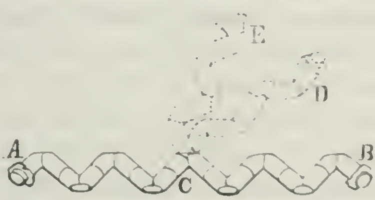


Fig. 2.

parts—one part, 4 for instance, serving as a polarizer and the remaining 5 as analyzer. The ends at *A* and *B* were capped with membranes and the whole filled with illuminating gas. Thus *AC*

may serve as a polarizer and *CB*, or *CE*, or *CD* as analyzer. When arranged as in *ACB* or *ACE* all conspire to the same effect of polarization; but when arranged as in *ACD*, the plane of all the L pieces in *CD* being at right angles to that of those in *AC*, the effect of one part antagonizes that due to the other, and to a maximum degree as regards the angle. Partial effects may be obtained with intermediate angles between 0 and 90°. Again, we observe that the L pieces of Fig. 2 may be alternately crossed, so that no two contiguous ones will be parallel. It is believed that this arrangement will give the greatest possible antagonistic effect; also, while all Ls are in one plane it is not necessary that they be arranged in a zig-zag line, like *AC* and *CE*, but may be indiscriminately connected in that plane. The few experiments made with the above-named arrangements gave very marked results. Of course it need not be confined to nine or any particular number of the L pieces.

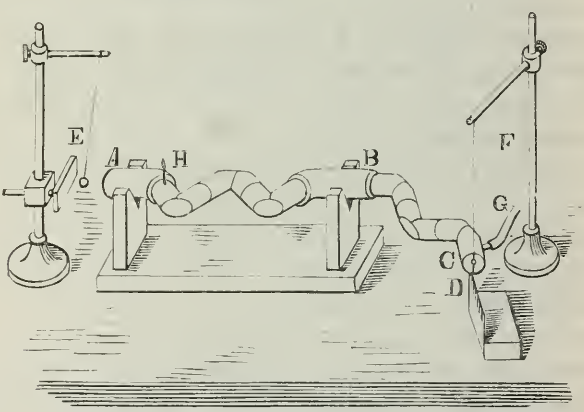


Fig. 3.

It was found, however, wanting in convenience. The apparatus finally adopted is that shown in Fig. 3. A different number of L pieces were used at different times. The portion *AB* is the polarizer and *BC* the analyzer. The joint at *B* was kept tight with beeswax; the ends at *A* and *C* were capped square with the same membrane material as were the angles of the Ls, giving, when charged with illuminating gas, a continuous zig-zag column from *A* to *C*. The L pieces of the polarizer enter half Ls at *A* and *B*, the latter having a common axis and resting in bearings at *A* and *B* in the standards, as shown. The object of this is to enable the experimenter to turn the

polarizer readily from cross to parallel, etc. This convenient arrangement of the polarizer is due to my assistant, Mr. Wright. Although applied to the polarizer, it is evidently equally applicable to the analyzer instead. The half L angles were not covered with membranes, but left solid, with gradual inside curvature. Membranes might have been applied here with partial polarizing effect. The half L solid angles are supposed to have detracted in a measure from the percentages of polarization obtained; but this sacrifice is more than compensated for by the greater convenience and constancy of conditions obtained. If this arrangement gives decisive results, of course more perfect apparatus would. The illuminating gas was admitted by a nipple and rubber hose at *C*, the same flowing the length of tubes and issuing in a small jet at *H*; my assistant kept this ignited, and used the flame length as a pressure indicator, and it served admirably.

The first trials were made by blowing an organ pipe in front of the membrane *A*, to agitate the gas column. A small mirror was attached to the membrane *C*, reflecting a pencil of light upon a screen. The deportment of the image indicated complex and inadmissible vibratory movements of gas column, and besides quantitative indication was found preferable to qualitative; thereupon the quantitative impulse and indicator pendulums were adopted, as shown at *E* and *F* respectively, Fig. 3. An ivory ball, ¼ inch in diameter, suspended by a thread of 8 inches length, was used at *E*, and so placed that when at rest the ball would just touch the membrane at *A*. The impulse was imparted by bringing the ball back against the stop, shown by means of a spatula held in the hand, and then allowing it to swing free against the membrane, each time with a definite predetermined arc. So much of the impulse as reaches *C* knocks the pendulum *F* through a certain arc, the same being measured on the scale *D*. This pendulum was a small, hollow glass bead, suspended by a silk fibre and trained delicately against the membrane. The bob carried a pointer for the scale *D*.

In the experiments the ball would be dropped against *A* some five or ten times, at intervals of about ten seconds, the corresponding deflections at *D* being noted and recorded; then the polarizer would be turned 90° and like observations noted. Again, 90° would be turned off, etc., etc.; occasionally the length of impulse arc would be changed, or more or less L pieces applied, and in each case a large number of observations made.

In the experiments the initial pulse seemed to be followed by a series of vibrations in rapidly decreasing amplitudes; but it is believed that the initial pulse is equivalent to a genuine sound wave, or an undulation. Evidence of soundness of this view is found in the fact that the velocity of sound can be satisfactorily determined by similar pulses sent through tubes of 25 or 50 feet length. It was evident that the initial pulse only was concerned in the first swing of pointer at D.

Results of Experiment.

Polarization of undulations by reflection from a surface separating coal gas and air. Signs = and + indicate relation of polarizer and analyzer, as in one plane or in perpendicular planes respectively.

	Gas in tube.			Air outside		
Series.	=	+	No. obs.	Per ct.	Ls in AB.	Ls in BC.
1	10·74	10·39	60	3·26	1	2
2	4.11	3·98	40	3·16	1	2
3	9·09	8·62	80	5·17	1	2
4	10·75	9·44	112	12·19	3	3
5	4·30	4·16	73	3·25	3	3

Total number of observations, 365.

Mean polarization, weights by number of observations, per cent. of unpolarized ray, 6·41.

Air both inside and outside of tube.

1	11·44	12·21	26	—6·73	1	2
2	8·34	8·40	64	—0·72	3	3

Total number observations, 90.

Polarization, per cent., 2·46.

The per cent. is obtained by dividing the difference of the values under the signs = and — by the value under =.

In obtaining the above results different impulse arcs were used, also different numbers of L pieces, as indicated in last column. The values under = and + are the divisions on scale D of swing of indicator pendulum, each being a mean of the number of observations named.

The results show a decided effect of polarization, though of smaller percentage than desirable. If the negative result obtained, with air in and out when it should have been nothing, is to be taken as due to some bias of the apparatus, it would be fair to add a like value to the polarized effect, thus changing the 6·41 to + 8·87.

But as a higher percentage was looked for, the instrument itself was now examined for possible faults; the membranes were all found under considerable tension, whereas, of course, they should be perfectly free from it. After completely slackening, then, as was supposed, the experiments were continued with the following results;

Polarization Continued.

Coal Gas in L tubes and Air outside.

Individual results.

Polarizer having 4 Ls and Analyzer 5 Ls.

	=	+	=	+	=	+	=	+	=
	6·0	6·0	8·0	5·0	6·5	5·8	6·2	6·0	6·0
	6·0	6·0	7·5	5·5	6·5	5·5	6·0	5·2	6·2
	6·2	6·0	7·0	5·3	6·3	5·6	6·5	5·2	7·0
	6·1	5·8	7·5	5·2	6·5	5·2	6·4	5·1	6·2
	6·0	5·7	7·0	5·0	6·2	5·7	6·3	5·5	6·1
	6·1	5·5	7·2	5·2	6·6	6·0	6·3	5·1	6·1
	6·2	5·8	6·5	5·0	6·8	5·2	6·2	5·2	6·1
	6·0		7·0	4·9	6·2	5·2	6·5	5·4	6·5
			7·0	4·0	6·3	5·3	6·3	5·8	6·8
							6·3	5·2	6·4
Means	6·07	5·83	7·19	5·01	6·43	5·50	6·30	5·37	6·34

In this series each value given in any column is the number of divisions on the scale *D*, Fig. 3, of the deflection of the indicator pendulum bob. After obtaining one column of results the polarizer, *AB*, was turned 90°, and the next column obtained. Thus the several columns were obtained.

On completing this series of observations air was passed into the L tubes, completely displacing the coal gas, so that the membranes were now suspended in mid air. Other conditions remained the same. The indicator pendulum now responded to the same impulses so slightly as to be barely observable, but not measurable, and they were apparently the same for the polarizer in all positions; that is to say, when air was upon both sides of the reflecting surfaces there was almost no appreciable reflection. This evidently should be the case, as the light membrane itself is now the chief cause of reflection. This, compared with the results obtained with gas in the tubes, shows that a considerable reflection is due to surfaces of separation of gases differing in density.

As regards the polarization, we observe that every mean under "=" is larger than any mean under "—", a fact which cannot be assumed accidental, nor explained on any other ground than polarization. To find the percentage of this in a single result, we have

Means of the above series.

	=	+
	6·07	5·83
	7·19	5·01
	6·43	5·50
	6·30	5·37
	6·34	
Means,	6·47	5·43

$$\text{Per cent.} = \frac{6{\cdot}47 - 5{\cdot}43}{6{\cdot}47} = 16{\cdot}1.$$

Whole number of observations, 80.

At this point the membranes were again examined and found to have appreciable tension, though supposed to have been entirely slackened at the beginning of the series of observations just cited. It was thereupon determined to slacken them with the utmost possible care, and continue the observations. The following table of individual results was then obtained in the manner explained above:

POLARIZATION CONTINUED.

Coal Gas in tubes and Air outside of tubes.

Polarizer having 4 Ls and Analyzer 5 Ls.

Individual results.

	=	+	=	+	=	+	=	−	=	+	=	+	=	+
	1·2	0·9	2·0	1·0	1·5	1·0	1·6	1·0	1·8	1·4	1·5	0·9	2·0	1·0
	1·5	1·0	2·1	1·2	1·5	1·0	1·4	1·1	1·5	1·2	1·5	1·0	2·1	1·2
	1·5	1·0	1·8	1·1	1·8	1·0	1·8	1·1	1·5	1·4	1·5	1·0	1·8	1·1
	1·5	1·0	2·0	1·0	1·3	1·0	1·9	1·2	1·7	1·2	1·5	1·0	2·0	1·0
	1·5	1·1	2·0	1·0	1·3	1·0	1·9	1.0	1·7	1·1	1·5	1·1	2·0	1·0
		1·0	2·0	0·9	1·2	1·0	1·8	1·0	1·7	1·2	1·5	1·0	2·0	0·9
			2·1	0·9	1·3	1·0	1·6	1·0	1·8	1·0			2·1	0·9
					1·5	1·0		1·1	1·7	1·0				
								1·1	1·7					
								1·1	1·6					
Means,	1·44	1·00	2·00	1·01	1·45	1·00	1·71	1·07	1·67	1·19	1·50	1·00	2·00	1·01

For gas displaced by air; other conditions the same; deflection, 0·00; polarizer turned 90° immediately following each column of results.

The smallness of these results, compared with those of the previous series, may be explained on the ground of extreme and entire slackness of the membranes; also the slackness is still further evinced by the fact that, when air displaced the gas no deflection of the indicator pendulum was observable in response to the impulses. Tense membranes would have turned the sound waves somewhat, and in the manner of the rebound of a drumstick.

To obtain the percentage of polarizing effect, we have:

Mean of the above series.

	=	
	1·44	1·00
	2·00	1·01
	1·45	1·00
	1·71	1·07
	1·67	1·19
	1·50	1·00
	2·00	1·01
Means,	1·68	1·04

$$\text{Per cent. } \frac{1{\cdot}68 - 1{\cdot}04}{1{\cdot}68} = 38{\cdot}1.$$

These results establish the following facts for sound waves or for undulations, viz.:

1st. A decided reflection occurs at a surface separating two gases of different density, confirming the views of Henry and Tyndall in this regard.

2d. In repeated reflection from such surfaces the intensity of the final component varies with the relative positions of those surfaces, the same following the laws of polarization in light, from which we conclude that longitudinal undulations can be polarized.

With sound polarized, we complete the list of effects for longitudinal undulations which are known to light, viz.: radiation, shadows, reflection, refraction, diffusion, diffraction, interference and polarization; and the laws are common for like conditions, viz.: for intensity of radiation in ambient space, $\frac{I}{d^2}$; in parallel space, $\frac{I}{d^1}$; in prismatic space, like a tube, $\frac{I}{d^0}$; for shadows, reflection, refraction and interference as well known; for diffusion, as when a steam whistle is sounded,

filling the air with its ring; for diffraction, as sound waves diverging rapidly after passing a narrow space between buildings, like light in passing a narrow slit and diverging; and, finally, for polarization, as above. In studying these comparisons we should recollect the vast difference between the properties of undulations in heavy and ethereal media. Thus the wave length is very great and the velocity of propagation very small in sound as compared with light. This seems sufficient to account for the greater definition of shadows in light; but when a slit is made as narrow for light as for sound, in comparison to wave length, the diffraction divergence is probably about alike; that is, the shadow of a silk fibre in light is much like a sound shadow of Bunker Hill monument, for instance. With these considerations it may be reasonable to expect incomplete or only partial polarization with such apparatus as employed above.

The conclusions to which we are conducted by the foregoing may be summed up as follows:

1st. That vibrations in extended media, produced from the action of a remote single centre of disturbance, can only be longitudinal, even in light.

2d. That vibrations will be to some extent transversal when due to two or more centres of disturbance not in the same line, as when two or more independent co-existent systems of undulations combine into one, or when a simple system is modified by such lateral disturbance as a reflection or a refraction.

3d. That undulations, to be in a condition called polarized, must consist of vibrations which are transversal, and that no necessity exists for assuming vibrations transversal in front of a polarizer.

[NOTE.—As regards longitudinal, oblique, transversal, etc., in the foregoing, the estimate is to be taken by comparing the direction of the line of vibration of a particle with that of propagation of the wave.

My acknowledgements are due to Mr. Clarence H. Wright, who, while a student in my physical laboratory last spring, rendered valuable aid in the experimental work. The arrangement of the polarizer as regards the *half* Ls, *A* and *B*, Fig. 2; and the bearings, whereby convenience is secured, is due to him.]

DESCRIPTION OF A MODEL OF THE GYROSCOPE FOR CLASS ILLUSTRATION.

By B. Howard Rand, M.D.

The gyroscope is usually spoken of as a toy. It is not such. It is as important an illustration of the law of centrifugal force as the Atwood machine is of that of gravitation.

It is seldom described in those gatherings of blunders known as "text-books;" and so much the better for the poor children who have to commit the statements (true or false) to memory. It is still more rarely shown to a class, perhaps because the "professor" does not understand it himself.

One great difficulty for the beginner in the study of mechanics is the erroneous definition of *inertia;* another, the misleading use of the words "tendency" and "tend," as applied to particles under the influence of force.

Inertia in the dictionaries and encyclopædias is used as synonymous with *vis inertia*. In the text-books it is described as a *property* of matter by which when at rest it "tends" to remain at rest, and when in motion to continue in motion in its original direction. That dead matter can exert force, as the term *vis inertia* requires, is absurd; that matter can "tend," is equally so. The mistake has continued from the fact that the compilers of text-books have copied the errors of their predecessors, and the teacher too often follows the book without independent thought.

Inertia is not a force, neither is it a property of matter. It is a convenient word to represent the fact that all change in matter, whether physical or chemical, is due to force. Matter, as we see it, is composed of many particles, hence the communication of the force requires time, in order to affect each particle in succession. Force is never lost, and a body influenced by it will remain under its influence until the force is neutralized or altered by other forces, either active or so-called passive. Among the latter are classed friction and resistance of the air by which the original force is converted into a new form, usually heat.

Perhaps the best illustration of the fact that the "inertia" of a body at rest is due to the law that the communication of motion to a mass requires time, is to be found in the starting of a long train of coal cars. The driver backs his engine until the "bumpers" touch

and the coupling links are slackened; then, starting the engine, it picks up one car after another, and thus puts the train in motion. Were the train "screwed" together, the adhesion of the driving-wheels would fail to give purchase enough to start it.

The "inertia of the plane of rotation," as seen in a boy's hoop or top, means simply that the original muscular force applied has to be exhausted before that of gravity can show its power.

The gyroscope is so well known that a description of it might seem needless. It may be worth the little time necessary to describe it to those who wish to follow the ideas of the model closely.

It is a smooth, heavy disc of metal on a shaft, which is suspended in a ring by two points at its ends, so as to permit of free rotation, Fig. 1. It is started by quickly drawing off a string, previously wound around the shaft after the fashion of a boy's top. The ring is firmly fastened to a rod, which has at its middle a cup, and on the other side of the cup a counterpoise weight, which can be fixed by a set screw so as to balance, underbalance or overbalance the disc. The cup rests on the point of an upright stem fastened in a base board; thus, hung like the ordinary magnetic needle, the whole is free to move in both a horizontal and a vertical plane.

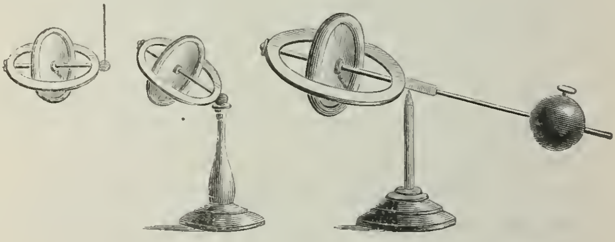


Fig. 1.

When the disc is balanced and the string pulled no movement, other than its own on its axis, is noticed. For convenience, it may be supposed to have right-handed rotation, to wit, that of the hands of a watch.

When the disc is not in rotation, if underbalanced it falls to the base-board or table, if overbalanced it is caused by the weight of the counterpoise to rise. Although these movements are not strictly in a vertical plane, they may, for the purpose of making the explanation shorter, be so considered.

When the disc, after being set whirling, is underbalanced, instead of falling it remains in the vertical plane, and at the same time a motion.

called "precessional," of the whole system around the upright stem is noticed. This precessional motion is more rapid as the velocity of the disc is greater. It is in an horizontal plane, and its direction is right-handed. Being in a plane at right angles to that of the disc, the motion is in a direction opposite to that of the upper edge of the disc. As the disc gradually, from friction of the bearings and of the air, comes to rest, the precessional motion diminishes, slowly ceases, and the disc falls. When overbalanced these movements are reversed.

The model which I submit is intended to explain these phenomena. It presupposes a knowledge of the principle of the composition and resolution of force. It can be made by any one of thick pasteboard, or at a small expense by a tinsmith.

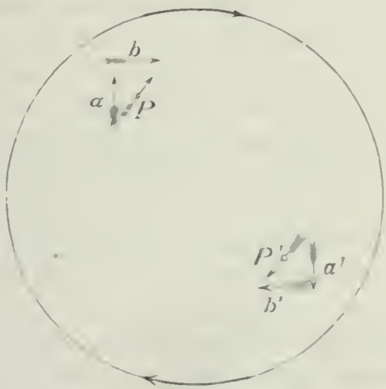


Fig. 2.

a a' vertical components. *b b'* horizontal components. *P P'* Particles under influence of the centrifugal force.

On the opposite sides of the disc, which is mounted on a stem with a counterpoise, are cut slots in the form of arrows, as represented in the cut, Fig. 2. In these slots are placed arrows, slightly smaller than the slots. These are hung by wire near the barb, so that they move freely through the slots. The arrow (*a*) on the left-hand side is weighted by solder at its butt, that on the right-hand side (*a'*) is weighted at its point. When the disc is tilted the arrows will still remain vertical, but with the points in opposite directions. The arrangement of the horizontal arrows will be explained later. The supposed particles, *p* and *p'*, indicated by dots painted on the disc, are under the influence of two forces; first, that of the arm used in drawing off the string. This impels them in the direction given

when the disc was in a vertical plane. Second, that of gravity, which pulls the disc towards the earth.

We have represented by the arrows the components, a and b, and a' and b' of the resultant motion of the two supposed particles, one near the upper edge of the disc, the other near the lower edge, and on opposite sides; they are moving in opposite directions at the same time. The direction of the rotation is shown by arrows painted on the disc near its upper and lower edges.

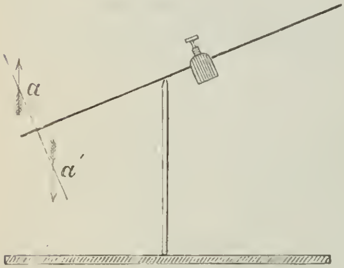


Fig. 3.

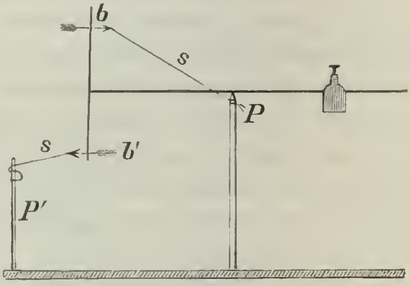


Fig. 4.

When the disc is tilted (Fig. 3) the vertical components remain vertical, like plumb-lines. The pressure of a upwards and a' downwards on opposite sides of the disc, due to their continuing in their first direction by virtue of the original force applied, will cause a movement around the point of support, s, on the known principle of a "couple," or bent lever. This can readily be shown by pressing at the same time on the front and back of the disc at the points indicated by the barbs of the arrows, the disc being counterpoised. The precessional motion will result.

The arrows representing the horizontal components are hung to the disc by a wire hinge near the point. They are attached by strings, s, to posts on the base-board, P and P', one in front of the disc, the other behind it. The holes for the strings are in the barbs of the arrows. The heads of the arrows indicate the pressure of the original force in opposition to an attempt to change the direction of the original vertical plane of rotation by the precessional movement. This can be shown by pressing near the upper and lower edges of the disc, as the arrows point, when the disc will be forced upward by a vertical force acting as a couple, or through a bent lever, as before,

Fig. 4. The neutralization of the force of gravitation acting on the disc is thus shown.

This explanation of the phenomena of the gyroscope is not claimed as original. The plan of the model for class illustration is believed to be so. The model is on deposit at the Franklin Institute, and any one is at liberty to make a copy.

SOUND FROM RADIANT ENERGY.

The announcement that Mr. W. H. Preece, the President of the Society of Telegraph Engineers, would lecture on the "Photophone and the Conversion of Radiant Energy into Sound," attracted a large number of auditors to the hall of the Institution of Civil Engineers on Wednesday, December 8th. This was partly due to the intrinsic interest of the subject coupled with the well-known ability of the lecturer, and partly to the fact that Professor Bell himself was expected to be present. Mr. Preece began his discourse by a reference to the other three great inventions of the last four years: the telephone, the phonograph, and the microphone, all of which, together with the latest marvel, the photophone itself, were concerned in the transmission and reproduction of sound.

After explaining that sound was a sensation caused by a vibratory motion in the air communicating itself to the drum of the ear, he illustrated the composite nature of light by throwing a splendid spectrum on the screen with the help of Mr. Ladd. Light, he observed, is a sensation of the eye set up by the action of a vibratory motion in the luminiferous ether. The telephone owes its action to an electric current varying in sympathy with the vibrations of the air constituting sound, and it is clear that if a substance could be got which, under the influence of light, would vary an electric current passing through a telephone, the transmission of sound to a distance by means of a vibratory beam would be rendered feasible. Such a substance is selenium, a body discovered in the year 1817 by Berzelius, when he was looking for tellurium, but it is so intractable in its physical properties that Professor Bell has had to overcome a great many practical difficulties in adapting it to his purpose.

We have so recently described the genesis of the photophone and its actual construction that it will be unnecessary for us to follow Mr.

Preece at length. We shall rather aim at presenting the most novel parts of his lecture and the fresh ideas it evoked in the discussion. The history of selenium is in a physical sense very interesting. It is an instance of a little known and somewhat ambiguous mineral, neither metal nor non-metal, which by virtue of a peculiar property, accidentally discovered by Mr. Willoughby Smith, has been chosen from among its fellows and exalted into honor. While seeking for a convenient form of high resistance to be used in cable testing, Mr. Willoughby Smith found that the exposure of a piece of selenium to light generated an electric effect in it which could either be attributed to a diminution of its electrical resistance, or to the creation of a current in the material. This original experiment was exhibited to the meeting by Mr. Willoughby Smith with the aid of a box which could admit or exclude the light from a piece of crystalline selenium connected in circuit with a battery and a reflecting galvanometer. The light of a lucifer match falling on the selenium was sufficient to deflect the light spot off the scale.

Mr. Willoughby Smith's discovery gave rise to the question whether the effect was due to the light, heat, or active rays of the spectrum, and whether it was a diminution of resistance or a new current excited. With regard to the first question, all subsequent investigators have shown it to be due to the light rays, Professor W. Grylls Adams and Professor Bell attributing it chiefly to the yellowish-green or middle rays of the spectrum, and Lieutenant Sale to the red rays. With regard to the second question Professor Adams has distinctly proved that a current is really set up in the selenium by the action of light, without a battery at all being in the circuit—a discovery confirmed by Mr. W. Crookes. The last mentioned investigator coated the vanes of a radiometer on one side with chromic oxide, and on the other with selenium, and found that in May of one year they rotated in a certain direction under the influence of light, while in October of the same year their rotation was in the opposite direction. In searching for the cause of this peculiar behavior he traced it to the use of a sperm candle giving a white flame and making the selenium surfaces to retreat on the one occasion, and the use of a wax candle giving a yellow flame making the chromic oxide surfaces to retreat on the other occasion. Here, then, we have two substances differently affected by rays of unequal refrangibility, the selenium being more sensitive to rays of higher refrangibility or smaller wave length. Mr. Robert Sabine's

experiments with selenium have established the fact that by the action of light on that substance an electromotive force is set up at the surface on which it falls; and Professor Minchin, of Cooper's Hill College, has demonstrated that light falling on one of two tin-foil plates immersed in hard water generates a comprratively powerful current. These observations tended to show that the effect of light on selenium was to generate an electric current; but other experiments by Mr. Sabine and others had proved that there was also a diminution resistance; and, as Professor Adams remarked in the discussion, both effects are produced together, the diminution of resistance being the stronger effect, and it is that upon which the theory of the photophone is based.

The intractable nature of crystalline selenium, and its extremely variable resistance, were first discovered by Dr. Werner Siemens, who found the variation of its resistence under the influence of light to be proportional to the square root of the illuminating power of the light: but when he attempted to construct a photometer on this principle he was met with disappointment owing to the uncertain resistance of the material. Two fragments of selenium from the same piece, prepared in precisely the same way, gave widely different results, and the same fragment altered from day to day. Professor Adams has observed that the resistance decreases by time, and instanced a piece which fell from 7,600,000 ohms in May, 1876, to 7·45 ohms in May, 1877. This liability to secular variation was one of Professor Bell's difficulties in working out the photophone; so, also, was the physical "fatigue" experienced by the mineral under the prolonged action of the light. This was early observed by Dr. C. W. Siemens, in experimenting with his "selenium eye," a simple apparatus consisting of a hollow ball containing a sensitive plate of selenium fixed in the focus of a lens in front of which are two movable shutters, these various parts corresponding respectively to the retina, the crystalline lens, and the eyelids of the human eye. The selenium plate is placed in circuit with a battery and galvanometer, and every time that light is admitted to the selenium by the opening of the eyelids, a corresponding deflection takes place in the galvanometer; the needle returning to its zero position when the light is again cut off. When the action is repeated often the selenium fails to respond as promptly as before, and the effect is lessened. This drawback had also to be overcome by Professor Bell. Various forms of selenium cell employed by him were illustrated by Mr. Preece.

The experiments which have led Professor Bell and Mr. Sumner Tainter to announce the discovery that all bodies are rendered sonorous under the influence of an intermittent or vibratory beam of light were very lucidly described by Professor Bell himself in opening the discussion. He explained how an intermittent beam of light, either luminous or deprived of its luminous rays by passage through a screen of ebonite, and allowed to fall on *thin* disks of any material, wood, paper, metal, gutta percha, glass, etc., caused the disks to give out a musical tone of a pitch depending on the number of intermissions per second, and of a *timbre* depending on the nature of the disk. Even tobacco smoke and crystals of sulphate of copper held in a test tube in the path of the intermittent beam emitted a corresponding tone, and even when the beam was simply focused within the outer ear itself, without any auxiliary appliances whatever, a note was likewise audible. In the short discussion which followed the lecture this conclusion of Professor Bell that all bodies are rendered sonorous by the fall of light in this manner, was pleasantly questioned by Dr. Tyndall, who thought it might be due to the expansion caused by the impact of heat rays; and in support of his remarks he cited an experiment which he had made in the presence of Professor Bell at the laboratory of the Royal Institution. He took the vapors of two volatile bodies, bisulphide of carbon and sulphurous ether, and exposed them successively in a glass vessel to the intermittent beam of light; and he argued that if the effect was due to the absorption of heat and the consequent expansion of the vapor, an audible effect would be obtained from the sulphurous ether, while on the contrary, the bisulphide of carbon would give no effect owing to the fact that it is almost completely transparent to heat. The result was that the sulphurous ether yielded a distinct musical note, and the bisulphide of carbon was quite silent. So far, then, as this experiment goes, Professar Tyndall's surmise is correct; but the subject will have to be cross-examined by a variety of careful experiments ere the question can be set at rest.—*Engineering.*

Progress of Electric Lighting.—We have already referred to the extensive sales of the Brush machine in England and America. A similar demand exists for other machines, and it is said that Siemens Bros. have more than 1000 of their machines now in use for purposes of illumination.—*La Gaceta Industrial.* C.

THE LATEST METHODS FOR PRODUCING PHOTO-TRACINGS IN BLACK AND COLOR.

Two new processes for taking photo-tracings in black and color have recently been published—"Nigrography" and "Anthrakotype"—both of which represent a real advance in photographic art. By these two processes we are enabled for the first time to accomplish the rapid production of positive copies in black of plans and other line drawings. Each of these new methods has its own sphere of action; both, therefore, should deserve equally descriptive notices.

For large plans, drawn with lines of even breadth, and showing no gradated lines, or such as shade into gray, the process styled "nigrography," invented by Itterheim, of Vienna, and patented both in Germany and Austria, will be found best adapted. The base of this process is a solution of gum, with which large sheets of paper can be more readily coated than with one of gelatine; it is, therefore, very suitable for the preparation of tracings of the largest size. The paper used must be the best drawing-paper, thoroughly sized, and on this the solution, consisting of 25 parts of gum arabic dissolved in 100 parts of water, to which are added 7 parts of potassium bichromate and 1 part of alcohol, is spread with a broad, flat brush. It is then dried, and if placed in a cool, dark place will keep good for a long time. When used, it is placed under the plan to be reproduced, and exposed to diffused light for from five to ten minutes—that is to say, to about 14° of Vogel's photometer; it is then removed and placed for twenty minutes in cold water, in order to wash out all the chromated gum which has not been affected by light. By pressing between two sheets of blotting-paper the water is then got rid of, and if the exposure has been correctly judged the drawing will appear as dull lines on a shiny ground. After the paper has been completely dried it is ready for the black color. This consists of 5 parts of shellac, 100 parts of alcohol, and 15 parts of finely-powdered vine-black. A sponge is used to distribute the color over the paper, and the latter is then laid in a 2 to 3 per cent. bath of sulphuric acid, where it must remain until the black color can be easily removed by means of a stiff brush. All the lines of the drawing will then appear in black on a white ground. These nigrographic tracings are very fine, but they

only appear in complete perfection when the lines of the original drawing are perfectly opaque. Half-tone lines, or the marks of a red pencil on the original, are not reproduced in the nigrographic copy.

"Anthrakotype" is a kind of dusting-on process. It was invented by Dr. Sobacchi, in the year 1879, and has been lately more fully described by Captain Pizzighelli. This process—called also "Photanthratography"—is founded on the property of chromated gelatine which has not been acted on by light to swell up in lukewarm water, and to become tacky, so that in this condition it can retain powdered color which had been dusted on it. Wherever, however, the chromated gelatine has been acted on by light, the surface becomes horny, undergoes no change in warm water and loses all sign of tackiness. In this process absolute opacity in the lines of the original drawing is by no means necessary, for it reproduces gray, half-tone lines just as well as it does black ones. Pencil drawings can also be copied, and in this lies one great advantage of the process over other photo-tracing methods, for, to a certain extent, even half-tones can be produced.

For the paper for anthrakotype an ordinary strong, well-sized paper must be selected. This must be coated with a gelatine solution (gelatine 1, water 30 parts), either by floating the paper on the solution, or by flowing the solution over the paper. In the latter case the paper is softened by soaking in water, is then pressed on to a glass plate placed in a horizontal position, the edges are turned up, and the gelatine solution is poured into the trough thus formed. To sensitize the paper, it is dipped for a couple of minutes in a solution of potassium bichromate (1 in 25), then taken out and dried in the dark.

The paper is now placed beneath the drawing in a copying-frame, and exposed for several minutes to the light; it is afterwards laid in cold water in order to remove all excess of chromate. A copy of the original drawing now exists in relief on the swollen gelatine, and, in order to make this relief sticky, the paper is next dipped for a short time in water, at a temperature of about 28° or 30°C. It is then laid on a smooth glass plate, superficially dried by means of blotting-paper, and lamp-black or soot evenly dusted on over the whole surface by means of a fine sieve. Although lamp-black is so inexpensive and so easily obtained, as material it answers the present purpose better than any other black coloring substance. If now the color be evenly distributed with a broad brush, the whole surface of the paper will appear to be thouroughly black. In order to fix the color on the tacky parts

of the gelatine, the paper must next be dried by artificial heat—say, by placing it near a stove—and this has the advantage of still further increasing the stickiness of the gelatine in the parts which have not been acted upon by light, so that the coloring matter adheres even more firmly to the gelatine. When the paper is thoroughly dry, place it in water, and let it be played on by a strong jet; this removes all the color from the parts which have been exposed to the light, and so develops the picture. By a little gentle friction with a wet sponge, the development will be materially promoted.

A highly interesting peculiarity of this anthrakotype process is the fact that a copy, though it may have been incorrectly exposed, can still be saved. For instance, if the image does not seem to be vigorous enough, it can be intensified in the simplest way: it is only necessary to soak the paper afresh, then dust on more color, etc.; in short, repeat the developing process as above described. In difficult cases the dusting-on may be repeated five or six times, till at last the desired intensity is obtained.

By this process, therefore, we get a positive copy of a positive original in black lines on a white ground. Of course, any other coloring material in a state of powder may be used instead of soot, and then a colored drawing on a white ground is obtained. Very pretty variations of the process may be made by using gold or silver paper, and dusting-on with different colors; or a picture may be taken in gold bronze powder on a white ground. In this way colored drawings may be taken on a gold or a silver ground, and very bright photo-tracings will be the result. Some examples of this kind, that have been sent us from Vienna, are exceedingly beautiful.

Summing up the respective advantages of the two processes we have above described, we may say that "nigrography" is best adapted for copying drawings of a large size; the copies can, with difficulty, be distinguished from good autograph, and they do not possess the bad quality of gelatine papers—the tendency to roll up and crack. Drawings, however, which have shadow or gradated lines cannot be well produced by this process; in such cases it is better to adopt "anthrakotype," with which good results will be obtained.—*Photographic News.*

Pictet's Ice Machines.—Pictet has read a paper before the French Society of Civil Engineers upon heat and the general theory of frigorific engines, in which he lays down several important laws and formulæ, and explains some details of the operation in various popular machines. In closing his communication, he invited the members to visit his works, where two ice-making machines are operating with sulphurous acid, one of which produces 1100 kilogrammes (2425 lbs.) of ice per hour.—*Ann. du Gen. Civ.* C.

Laws of Electro-Magnetic Machines.—J. Joubert finds that for a given intensity of field, under whatever other conditions the machine may operate, at the moment when it gives the maximum work the intensity is constant and equal to the quotient by $\sqrt{2}$ of the absolute maximum intensity; the electro-magnetic work is proportional to the velocity; the velocity is in a constant ratio to the resistance. The machines, therefore, differ from ordinary batteries, inasmuch as the battery maximum requires no external resistance—*Comptes Rendus.* C.

Deadening Vibrations.—An encouragement of 500 fr. ($100) has been awarded to M. Anthoni, of Levallois Perekt, for a plan for deadening the shocks and vibrations which result from the use of mechanical hammers, pile drivers, etc. It consists in the interposition of plates and washers of caoutchouc between the base of the machines and their foundations, so as to leave no contact between the metal and the ground, except through the intervention of elastic media. Some of his machines have been put in operation with encouraging results. —*Bull. de la Soc. d'Encour.* C.

Vines from Soudan.—Th. Lécard has found numerous botanical surprises in the immense and dangerous solitudes of Soudan. Among the most valuable novelties are a variety of wild vines, with fruit of a delicious flavor, herbaceous stalks, and perennial tuberculous roots. The beauty and abundance of the fruit, the vigor of the plant, and the ease of culture by simple plantation of the tubercles, lead him to hope that they may be acclimated in France, as a substitute for the vines which are destroyed by the phylloxera. He has procured a large quantity of seeds for distribution to the agricultural and scientific establishments of Algiers, France and other parts of Europe.—*Comptes Rendus.* C.

Dangers of Unequal Loads.—In a recent discussion before an architectural society, in Paris, one of the engineers attributed the fall of the roof of St. Martin market to the unequal distribution of the snow, which had been swept by the wind and drifted in some places to a depth that had never been anticipated. It can readily be seen that such drifts might break a weak purlin, and thus withdraw a part of the support of one of the beams, so as to lead to the destruction of the entire roof.—*Chron. Industr.* C.

Vibratory Forms of Circular Pellicles.—C. Decharme reports some experiments upon circular pellicles of soapy water, which are set in vibration by means of musical rods. He finds that for any given diameter of pelicle the number of nodes are inversely proportional to the corresponding lengths of the vibrating rods. This fundamental relation is the same as the one which he had previously discovered in the vibrations of bubbles, and all his equations appear to be applicable in both cases.—*Comptes Rendus.* C.

Mechanical Action of Light.—Charles Cros refers to a memoir which he addressed to the French Academy on the 20th of May, 1872, in which he anticipated, from the *action* of different media upon luminous rays, a *reaction* of light upon the medium. He proposed certain experiments for rapidly and regularly interrupting the passage of a ray, so as to produce audible sounds, as Graham Bell has recently done by means of the photophone.—*Comptes Rendus.*

[The control of æthereal vibrations by mechanical action appears to have been first demonstrated by Chase, in his experiments before the American Philosophical Society in 1864.] C.

Trouvé's Motor.—For several months M. Trouvé has made numerous experiments with his new motor, which he has attached to the hunting yawl *le Telephone*. The game, not being startled by the noise and movement of oars, allow the boat to approach them very nearly, so as to be captured in large numbers. At first the yawl had a velocity of 1·2 metres per second (2·68 miles per hour). After certain changes a velocity was obtained of 2 metres per second (4·47 miles per hour) when moving against the current, and of 2·5 metres (5·49 miles) when moving with the current. Much greater velocities are anticipated from the more powerful motors which are soon to be built.—*Chron. Industr.* C.

Wool Sorting.—Levoiturier, an entomologist of Elbeuf, has prepared a long list of the coleoptera which he has found in the wools of different regions of the globe, and which he has classified so as to show those which are peculiar to special regions. This classification has rendered an important service to the wool industry, since a simple inspection will often enable the sorter to decide upon the origin, and consequent value, of samples which might otherwise be in doubt.—*Les Mondes.* C.

The Telephone in China.—The Chinese alphabet is so peculiar that there is great difficulty in devising any practicable system for conveying telegraphic messages. The telephone, therefore, is received with peculiar favor by the Chinese government, which has at length decided to establish a complete system of telephones throughout the country, commencing north of Yang Tse Kiang. The work will be conducted under the charge of J. A. Betts, the American telegraphist, under whose superintendence the telegraphic line was built from Tien-tsin to Taku.—*L'Ingen. Univ.* C.

Application of the Photophone to Solar Explosions.—Abbé Moigno suggests the possibility that the photophone may become a means of intercourse between individuals upon different planets, or even in different systems. Graham Bell, having been invited by Janssen to the observatory of Meudon, examined with great care the photographs of the sun's surface. Janssen having stated that they gave evidence of movements of a prodigious rapidity in the photospheric materials, Bell conceived the idea of employing the photophone for the reproduction of the sound which must necessarily arise from the explosions.—*Les Mondes; Comptes Rendus.* C.

The Odors of Paris.—H. Sainte Claire Deville has analyzed samples of the dark soil which immediately underlies the pavements of Paris, and finds that the odors which arise from it are in no wise injurious, on account of the empyreumatic and antiseptic products which are constantly introduced by the illuminating gas that escapes from the street mains. This is not the case, however, with the sewage material, from which disagreeable odors are constantly exhaled in many parts of the city and its environs. He anticipates from Pasteur's investigations some early discoveries which will relieve them from their cholera producing and typhoid tendencies.—*Comptes Rendus.* C.

Book Notices.

A History of the Jetties at the Mouth of the Mississippi River (Jas. B. Eads, Chief Engineer). By L. C. Corthell, C.E. Chief assistant and resident Engineer during their construction. 8vo, New York: John Wiley & Sons, 1880.

History is composed of battles, and this history portrays *some* antagonisms and exults in *some* victories, whose memories could have been left without an epitaph, with not a little propriety. As a narrative of engineering accomplishment, and an example of controlling a river upon a large scale, the history of the progress, day by day, of the work in overcoming original obstacles, in meeting developed ones, and finally in attaining success, the book becomes an interesting addition to engineering literature.

The great value of the work rests upon its being a full account, with maps and charts, of the Mississippi river. A discussion of the application of the system of controlling currents of rivers by jetties is also given, with a statement of examples in other lands, and a tolerably complete description of the appliances for effecting the work: dredgers, pile drivers, apparatus for preparing concrete blocks, etc., as well as the material of jetty building, mattresses, cribs and cement blocks and their manipulation. In these regards, this work of Mr. Corthell becomes a piece of the standard literature of modern engineering, and demands a place in every library of the class. R. B.

Searles' Field Engineering. A hand-book of the Theory and Practice of Railway Surveying, Location and Construction. William H. Searles, C.E. 12mo. New York: Wiley & Sons, 1880.

At once the most profitable and the most useful technical literature, is that which has adapted science to common use; and the text-book, or hand-book, which bridges over the chasm between the learning of the schools and the applications to practice, affording either knowledge or aiding memory, supplies an essential and constant want. Such books are necessarily incomplete in elementary information, incomplete in logical deduction, incomplete in universal application; but in all these regards of information, deduction or application they need not be erro-

neous. In fact, their end is best served when they are comprehensible, correct in statement, and suited to meet *usual* demands in the particular technical field to which they appertain. And this particular handbook possesses, in a great measure, the merits belonging to the best of the class.

In one special regard this book deserves commendation; it is not attempted to load the pocket of the user with a folio of instruction in simple arithmatic and elementary mathematics. The violent assumption that a man—the reader—must *know* these, before he sets up for a civil engineer is actually asserted in the first page of the preface! And the book at once proceeds to discussions and problems in the practice of railway construction, such as one desires to refresh his memory about, after years of disuse and forgetfulness, when the emergency arises.

In some points the tables are wanting in convenience in taking out figures. Clustering 50 lines of figures to each page, as is universal in German tables (100 lines for two facing pages), is highly advantageous. The table of logarithms of numbers would have been more useful for quick reference and field use if it had been restricted to four-figure numbering and five-figure logarithms, while for office use, the longer tables of five numbers and seven-figure logarithms is indispensible. But objections to these tables would measurably disappear as one becomes habituated to the use of them. The book needs an *index.*

We know of no compendium so convenient for ready reference, and none where tabular information has been made so dependent on the text and equally convenient to apply without chance of mistake. R. B.

THE PRINCIPLES OF THERMO-DYNAMICS, with special Applications to Hot-Air, Gas and Steam-Engines. By Robert Röntgen, Teacher Polytechnic School, Ramscheid. Translated, revised and enlarged by A. Jay du Bois, Professor Yale College, New Haven, Conn. 8vo. New York: John Wiley & Sons, 1880.

Possibly it would have been better if the title were to have acknowledged the sources of principal enlargement, Prof. Verdet, M. Pernolet, Prof. Zeuner and others, as is admitted in the preface.

The work itself is a collection of knowledge on the subject of thermo-dynamics, translated and prepared by Prof. du Bois for the use of technical classes in colleges and polytechnic institutes. While a logical course of instruction is indicated to the student by a series of questions for examination, which have been appended to each chapter.

From the practical point of view it may be remarked that too much prominence and space is given to numerous special cases of mechanism for hot-air and gas-engines, many, if not most, of which are now merely historic curiosities, while the discussions of higher types of steam-engines, which have developed its real working existence, are imperfectly considered. The real applications of thermo-dynamic properties give sufficient grounds for study, so that the curiosities of proposition, or of startling, but unproductive, demonstration, may be set aside as a portion of the pleasant reading, "the diversions of Purley," for the learned, who have become surfeited with elementary knowledge. A chapter on cold-producing machines, for example, would become a practical study, sufficiently difficult to test the reasoning abilities of a class, and would carry with it the inculcation of positive knowledge.

Whatever might, or can, be the method or argument or course of examples of a perfect text-book on thermo-dynamics (which study ought to be one of a series of mechanical text-books, with references and cross-references throughout the series), it is certain that Prof. du Bois has collected the most thorough and practical work in the English language to this time; and that its study is made simple and easy to the mechanical engineer who has the applications of heat as one of the demands of his practice. R. B.

ON ANGULAR APERTURE OF OBJECTIVES FOR THE MICROSCOPE. With Appendix and 18 full-page Illustrations. By Geo. E. Blackham, M.D., F.R.M.S. 8vo. New York: The Industrial Publication Company, 1880.

The publication before us is the reprint of a paper read by Dr. Blackham before the Microscopical Congress at Indianapolis, Ind., Aug. 15th, 1878, and is worthy to be on the table of every working microscopist. It is rarely that we meet with so clear and concise an exposition of so difficult and much disputed a subject as we find in this treatise, and we cannot but congratulate the author on the happy omission of all mathematical formulæ and calculations, which so frequently only bewilder the student of microscopy who is not an accomplished mathematician. In plain and simple language, and by the aid of excellent diagrams, we think the author has succeeded in making an intricate subject clear enough for any one to understand. The book is elegantly bound, and the press-work as well as the illustration do great credit to the publishers. C. S.

INTRODUCTION TO THE STUDY OF CHEMICAL REACTIONS. By Dr. P. E. Drechsel, Prof. Physiol. Chem. at Leipzig University. Translated by N. F. Merrill. 12mo. New York: John Wiley & Sons, 1880.

This book we confidently believe will prove of great value to the student of theoretical chemistry. It is well adapted as a companion to the usual college text-book, and gives a lucid and comprehensive explanation of chemical reactions, according to the most recent views of modern chemistry. Its exposition of the rationalistic formulæ is the most satisfactory we have seen in any text-book. R. H.

EASY LESSONS IN SANITARY SCIENCE. By Joseph Wilson, M.D., Medical Director U. S. Navy. 8vo. Philadelphia: Presley Blakiston, 1880.

Again it devolves upon us to chronicle the advent of a work upon that much-agitated subject, sanitary science. It might be construed as a favorable omen by optimists that such an amount of interest is manifested in a science which has for its object the increase of the happiness of the race, and it is not without some force of reasoning that discovery and invention are regarded as potent factors in promoting peace and comfort, inasmuch as the same mental activity which contrives those destructive engines which make war so terrible that nations hesitate to invoke hostilities to adjust their differences, also devises means to combat and exterminate those insidious and invisible enemies which assail us from various sources, and against which we have hitherto considered ourselves powerless.

The design of the little work under review appears to be to take the place of those "Primers" which supply such an important want in other sciences, namely, an insight into the broad principles underlying each department of knowledge, in order that those who cannot find time to go further may at least avoid entire ignorance—for it is now admitted that a little knowledge, far from being dangerous, is often of the utmost value, besides being all the best informed can boast of, upon many subjects.

The first half of Dr. Wilson's work is devoted to the subject of the drainage of land, and its influence upon its productiveness and the health of its inhabitants, illustrating his conclusions by circumstances

which have come under his own observation, which, although somewhat unnecessarily amplified, nevertheless serve to prove his statements to be correct.

The next subject considered is house drainage, its defects and their remedies being set forth by most appropriate cuts in connection with some important suggestions.

The writer then hastily presses on to review the drainage of cities and the important problems involved, and incidentally calls attention to the danger of drinking the water of rivers polluted with sewage, owing to the possibility of imbibing the ova of some of the entozoa infesting the human body, and suggests, as a possible remedy, wells, or filtering galleries situated adjacent to the bank of the river, from which water might be drawn freed from its impurities.

With some desultory remarks upon defective plumbing the work closes, and we think that, taken as a whole, it is probably more calculated to attract the general public than any hitherto published upon this subject. W. B. C.

Electric Lighting by Incandescence, and its Application to Interior Illumination. With 96 illustrations. By William Edward Sawyer. 8vo. New York: D. Van Nostrand. 1881.

This is a book of great practical value to all who wish to understand the subject. The style is exceptionally clear and comprehensive. We of course perfectly understand that the book is written in order to introduce the Sawyer system of lighting to the general public; therefore the closing words of the Preface, implicitly carried out as they are throughout the whole book, appear noble by comparison with many books of the same order. We quote: "Those who expect to find them devoted to criticism of the labors of other experimentalists will be equally disappointed. In the position of an impartial student and observer, I have sought less to indicate defects than to exhibit accomplishments."

Of the work 53 pages are devoted to a full description of all the types of dynamo machines, of which only ten describe the Sawyer. Thirty-three engravings fully illustrate this portion of the book. A view of each machine, with diagrams of internal construction in detail are given, and explained in the text with unusual clearness and perspicuity. Especially is this the case in the winding of the wires in the various methods of construction, so that any intelligent mechanic

could, from the drawings and text, easily construct any of these machines. The various physical laws are introduced incidentally, as occasion requires. The writer has not seen any book from which so much detail can be learned as from this.

As the Sawyer light is of the incandescent class, of course the book treats of that class in particular, devoting 61 pages of text and 32 engravings to the lamps and carbons, nearly one-half of which is taken up with other patents. The details of the Edison alone are illustrated by 7 diagrams, giving the best idea of the process of manufacturing the Edison lamp we have seen; also, a fine view of the Maxim form. As a part of the lamp, 20 pages and 5 plates are devoted to the manufacture and preservation of carbons. A page diagram is given of the Sprengel pump, as used by Edison and others. All conflicting patents appear to be given, with dates. The chemical notes beginning page 105 are excellent. His remarks, page 103, upon the comparative cost of arc and incandescent lighting present the subject so clear that we quote: "Light by incandescence is considerably more costly than light by the voltaic arc, when the volume of light obtainable is the sole consideration. The same expenditure of power that will produce a light of 1,000 candles by the voltaic arc will not produce, on an average, more than half or one-third as much light as incandescence in a divided circuit. It should not, however, be forgotten that the power of any light decreases as the square of the distance from it, and that one-fourth of the light of the arc, distributed at four or five appropriate points, thus reducing the power of each light to $\frac{1}{16}$ of that of the voltaic arc, will give substantially as good a general illumination as the arc. The incandescent light is whatever may be desired. The arc light is necessarily a powerful one. The objection to it, if used without a shade, is its great intensity and ghastly effects; and in order to obviate these defects, glass shades of more or less opacity are employed, which, according to tests, involve a wastage in light of,

With ground glass, . .	. 30 per cent.
With thin opal glass, . .	40
With thick opal glass, .	. 60

In some cases wastage is nearly 75 per cent."

Experiments in France on the Jablochkoff, with the necessary opalescent globes, "it is found that only 43 per cent. of its full power is available." "It is proper to remark that the light of the incandescent carbon is very unlike that of the voltaic arc. Its characteristics

are the characteristics of daylight; and this is true to such an extent that, from its soft and agreeable nature and absence of glaring effects, the degree of illumination afforded is not always readily appreciated." Besides, incandescent lights *do not* require shades.

Chapter 9 treats of "the division of the current and light;" a most interesting chapter because of the widely different opinions of physicists upon the subject. This should be read by all who are interested, as the author states his case and calculations clearly. If verified by future experiment they are of great value, but only actual experiment can decide.

Chapter 10 is devoted to a description of the Sawyer regulator, called here the switch—a most important part of the system, as by it the light is almost instantly turned up or down, giving any degree of intensity of light as required by the person using it. The subject is illustrated in 10 perspective and 6 diagram views, including the current regulator, one being a fine view of the Maxim governor. While these regulators admirably perform their functions, he is careful, in perfect candor, to inform the reader (page 149) that, "By means of these regulators the changes in the circuit occasioned by the Sawyer switches for graduating the light are instantly balanced. But the fact remains that as much power is expended in driving the generator when there are a few as when there are many lamps in circuit, and in a general distributing system, where economy is the prime consideration, such regulators, however perfect in their operation, can have no practical application." We venture to remark that this is not a peculiar defect of the electric light, but is a foundation principle governing every other industry.

Chapter 11 is devoted to the consideration of the Sawyer patent for lighting the buildings in the blocks of a city by electric lights. The history, plans, meters, switches, etc., requisite are fully set forth in the text and 11 diagrams. It also deserves careful study. In it the "Niagara Falls problem" is discussed at length, and, to our mind, settles that question.

The last chapter is devoted to the commercial aspects of the subject—that omnipotent question, "Will it pay?" This chapter involves so many points, exhibits so many tables and calculations, which must be most carefully studied before an opinion can be given, that we have not time this month to devote to it. It is in print, in the book, for all interested to criticise. E. F. M.

Franklin Institute.

HALL OF THE INSTITUTE, February 16th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 107 members and 85 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced, that at their last meeting, 18 persons were elected members of the Institute. Also that the Board had awarded the Elliott Cresson Gold Metal to Louis H. Spellier for his invention of the Electric Time Telegraph in accordance with the recommendation of the Committee on Science and the Arts.

The Secretary announced that at a meeting of the Committee on Science and the Arts, held February 2d, 1881, William Dennis Marks, Whitney Professor of Dynamical Engineering in the University of Pennsylvania, was elected chairman of the committee for the present year.

The following donations to the Library during the past month are reported:

Specifications and Drawings of United States Patents from March to June, 1880. From the Commissioner of Patents.

Reports of the State Board of Agriculture, etc., for 1879. From J. W. Huttinger.

The Locomotive. N. S., Vol. 1. 1880. From J. M. Allen.

Report of Light-House Board for 1880. From the Board.

Annuaire de l'Academie Royale de Belgique, 1881. From the Academy.

Electric Lighting. By W. E. Sawyer. From the Author.

Report on Standard Gauge for Bolts, Nuts, etc., for U. S. Navy.
Report on Machinery of the Steamer *Anthracite*.
Report on Experiments Tried with Horizontal Fire-tube and Vertical Water-tube Boilers.
Researches in Steam Engineering. By B. F. Isherwood. Vol. 1. From B. H. Bartol, Philadelphia.

Catalogue and Charts of the United States Coast and Geodetic Survey.
From Edward Goodfellow, Assistant in Charge of Survey Office.

James Smithson and his Bequest. By Wm. J. Rhees. From the Smithsonian Institution.

Papers read before the Pi Eta Scientific Society. Troy, N. Y.
From the Society.

Lecture on "Ye Microscope of ye Olden Time." By E. F. Moody.
From the Author.

Historical Sketch of Oliver Evans. By Rev. G. A. Latimer.
From the Author.

Thirty-fifth Annual Report of the Director of the Astronomical Observatory of Harvard College. By E. C. Pickering.
From the College.

Report on Machinery of Steamer *Anthracite*.
From the Navy Department, Washington.

Annual Report of Secretary of Treasury on Finances for 1880.
From the Secretary.

Textile Industries of Philadelphia. By Lorin Blodget.
From Chas. Bullock.

Catalogue of Brooklyn Mercantile Library. Part 3.
From the Library.

Catalogue of the Library of the Institute of Actuaries. Nov., '80.
From the Institute.

Catalogue and Address of the President of Penna. State College, 1880–81.
From the College.

Report of the Commissioner of Agriculture for 1879.
From the Commissioner.

Reports from the Consuls of the United States on Commerce. No. 2.
From Secretary of State, Washington.

Reports of Commissioner of Education for 1878.
From the Commissioner.

Transactions of Institute of Mining Engineers. Vol. 8.
From the Institute.

Report of Meteorological Council to the Royal Society for 1880.
From the Society.

Circular of Information of Bureau of Education. Nos. 4 and 5. 1880.
From the Commissioner.

Gold Standard. By Wm. von Kardorff-Wabnitz.
From H. C. Baird & Co., Philadelphia.

Report of Proceedings of Numismatic and Antiquarian Society of Philadelphia for 1880.
From the Society.

Geological Survey of Canada. Report of Progress for 1878–9.
From the Survey Department.

Abridgements of Specifications of British Patents relating to
Dressing and Woven Fabrics. Pt. 2. 1867–76.
Manufacture of Iron and Steel. Pt. 2. 1867–76.
Electric Lighting, etc. Pts. 1 and 2. 1839–76.
Umbrellas, etc. Pt. 2. 1867–76.
Letter-press, etc., Printing. Pt. 2. 1867–76.

Disclaimers, 1874, No. 2734; 1875, No. 1204; 1877, Nos. 788, 3205, 4200; 1878, Nos. 688, 5247; 1879, Nos. 2138, 3300 and 4761.

Specifications and Drawings of Patents for Inventions. Vols. 34 to 51. (Nos. 3301 to 5100. Aug. 15 to Dec. 12, 1879.)

From the Commissioner.

The President named the following Standing Committees of the Institute for 1881:

On the Library.—Chas. Bullock, Lewis S. Ware, Dr. Isaac Norris, Robert Briggs, Henry Bower, Henry Pemberton, J. E. Mitchell, Jos. M. Wilson, Fred. Graff, Dr. W. Lehman Wells.

On Minerals.—Dr. F. A. Genth, Theo. D. Rand, Clarence Bement, Persifor Frazer, Dr. W. H. Wahl, E. J. Houston, Otto Luthy, E. F. Moody, Dr. G. A. Koenig, H. Pemberton, Jr.

On Models.—C. Chabot, H. L. Butler, Edward Brown, M. L. Orum, J. Goehring, L. L. Cheney, J. J. Weaver, S. Lloyd Wiegand, A. G. Busby, N. H. Edgerton.

On Arts and Manufactures.—J. J. Weaver, George V. Cresson, Hector Orr, Coleman Sellers, Jr., W. B. LeVan, Wm. Helme, J. S. Bancroft, Alfred Mellor, Cyrus Chambers, Jr., Geo. Burnham.

On Meteorology.—Pliny E. Chase, Hector Orr, Dr. Isaac Norris, David Brooks, Jas. A. Kirkpatrick, Alex. Purves, Dr. W. H. Wahl, F. M. M. Beale, H. Carvill Lewis.

On Meetings.—Fred. Graff, Washington Jones, Chas. H. Banes, A. E. Outerbridge, Jr., W. L. Dubois, W. H. Thorn, Cyrus Chambers, Jr., J. J. Weaver, Addison B. Burk.

A letter was read from Mr. Dalton Dorr, the Secretary of the Pennsylvania Museum and School of Industrial Art, returning a vote of thanks, from its Board of Trustees, to the Institute for the joint occupation of the rooms of the Drawing School, and regret that the increase in the number of the pupils of the schools prevented its continuance.

The Secretary's report included Edisons Electric Lights, brought on

from Menlo Park for exhibition before the Mining Engineers at their annual meeting, held this year at the Franklin Institute. There were about thirty lights, some of them rated at full power (sixteen candles), others at half and one-third power, the latter being for miners' use.

Prof. Moody, who had charge of the lamps in the absence of Dr. Moses, said that we had all heard of the wonderful things that Edison had been doing, or was going to do, and we now had an opportunity to see some of the fruits of his labors. Edison's purpose is not to produce a light of great power such as we are accustomed to see, but small lights equal to a single gas jet, for household use, and lights of one-half or even smaller size, for use in mines. There are two systems of electric lighting—one by the voltaic arc, and one by incandescence, the latter being best suited for household use. The arc lights are well represented by the Brush lights now in use for illuminating Broadway, New York, and the incandescent system is represented by the lights of Sawyer and Edison. Prof. Moody described the process of making the Edison lamps, stating that the carbon loops were now made of bamboo fibre, and were contained in an almost perfect vacuum. The resistance of the larger lamps is about 100 ohms, that of the half lamps 50 ohms, and of the others 30 ohms. The dynamo machine belonging to the Institute, which is used this evening, is not well adapted for the Edison lights. At 800 revolutions per minute, the normal rate at which it is run, the internal resistance is too great. Still it could be used with the lamps by increasing the number of revolutions to 1000. Prof. Moody also discussed briefly the different characteristics of light from gas and from electricity, saying that the latter appeared to have greater power of penetration, as shown by experience at the Hoosac tunnel.

Mr. Burk said that he could give no further information about Edison's light than he had obtained from the inventor himself. There was great difficulty originally in making the lamps uniform in resistance, and without uniformity in this respect they could not be used to good advantage on a common circuit. He had been informed, however, that the lamps could be made now with reasonable certainty, that when their resistance was measured they would fall within one of three classes—one class having a resistance of from 100 to 105 ohms, another of from 105 to 110, and the third of from 110 to 115. It was proposed now to assign one of these classes of lamps to New York and other cities, one to Philadelphia and other cities, and another to Chi-

cago, etc., and so to confine each city to lamps of a particular resistance. In this way the whole product could be utilized, and uniformity in lamps of the same circuit secured.

Mr. Orr expressed a desire to have this whole subject of electric lighting thoroughly investigated by the Institute.

Mr. Cooper inquired whether the lights as shown were as white as they could be made. He thought that they gave forth a light with more red in it than was to be seen in gas, and said that in this respect the Sawyer light seemed to him superior.

Mr. Burk said that the color depended altogether on the degree of incandescence. The light can be run up very much higher than it is shown here, and with a higher degree of incandescence it becomes whiter. But there is a limit of safety for the lamp, and it is dangerous in a public exhibition of this kind, with no regulator of the current, to run them too close to their limit, for fear of breaking the lamps and of doing an injustice to the inventor by making an apparent failure, due, not to an imperfection in the lamps themselves, but to an abuse of them. He had seen the lights at Menlo Park much brighter than these, but there the current was steadier and under perfect control. The Edison light could certainly be made as white as any other under the incandescent system, that depending wholly on the current supplied; as to whether the higher lights could be produced economically Mr. Burk had no opinion to express.

The sand blast apparatus for sharpening files was on exhibition, and its effects were illustrated by magnified sections of files as they appear when new, when worn, and after having been sharpened by the sand blast process. The apparatus consists of a steam pipe and injectors by which to drive a stream of sand and water against the files to be sharpened, the latter being carried into the blast on a carriage. The injectors are set above and below the file at an angle of fifteen degrees with it. The operation consists of simply turning on the blast, when the sand cuts away the backs of the teeth until they are brought to a cutting edge. The inventor claims that files can be sharpened by the sand blast without previous preparation and without the necessity of tempering them afterwards, as the process does not affect the temper, and that they can be sharpened by the hundred at a cost of about three cents each. One special use of the blast is the removal of the burr formed by the chisel in cutting the teeth, and which has to be worn down before a file is put in good condition.

R. D. Wood & Co. exhibited Matthews' Double Valve Fire Hydrant, which is ingeniously devised to overcome ascertained objections to the common fire plug used in this city. The new hydrant has a frost-jacket, extending from the bend at the bottom upwards to a few inches above the side-walk, and forming a "dead air chamber," to act as a non-conductor. The casing has a vertical play or motion, so that the hydrant proper, which is not pressed upon by the surrounding earth, is left undisturbed—even when the jacket is lifted by the upheaval of the ground by frost. The casing also allows the hydrant to be removed for repairs without digging up the ground. Two main or induction valves are used, and they are so connected together with the waste valve that the latter is never opened until the lower main valve is closed, and is closed before the lower main valve is opened. The movement is positive, and while the valves are in order there can be no leakage or waste. The chief advantage, however, from the use of two induction valves is that, in case of injury to the upper (usually the main valve), the latter can be taken off and the hydrant returned to its place while repairs are being made, the lower valve then becoming the main valve and keeping the hydrant ready for use. The inventor claims that the efficiency of the Fire Department of this city would be greatly increased by the use of these hydrants in place of the old-fashioned plugs, that freeze every winter, and are in constant need of repairs.

Thos. S. Speakman exhibited drawings of apparatus designed to utilize the caloric of heated gases of the products of combustion now allowed to escape up the chimney. His plan involves the use of collecting and condensing chambers, and of a blower or pump. The gases are passed through a condenser immersed in water, which latter, being heated, is used for feed water of the boilers, thus economizing fuel, while the products of combustion are separately collected, so that the carbon may be utilized as such, and the animal matter prepared for fertilizing purposes.

Mr. H. B. Hart exhibited the "Extraordinary Challenge Bicycle," which differs from the ordinary form in having the rider put farther back than usual over the rear wheel, which is made larger, to sustain the greater weight thus put upon it. The change in position of the rider makes it necessary to use levers in the propelling gear, but the pedals are directly under the saddle, and describe an ellipse instead of

a circle, so that a more direct and downward pressure is exerted than in the ordinary bicycle, which overcomes the loss of power resulting from use of more connections. It is claimed that this bicycle is safe, easy to mount, and fitted for riding over rough roads. It was on machines of this kind that two prominent gentlemen of this city rode last fall from Saratoga to Philadelphia, making the trip leisurely in a week.

A gas measure indicator, invented by Mr. John Manuel, was also shown. It is intended to be attached to the meter cock on the supply pipe, either before or after passing the meter. It is a regulator of the flow of gas, and the quantity consumed is ascertained by calculation of the number of burners of known size used under a known pressure for a definite number of hours.

Attention was called to a twin gas burner, in which two jets of gas are made to impinge on each other. The flame is round, and without so much of the dark centre usually found in gas lights.

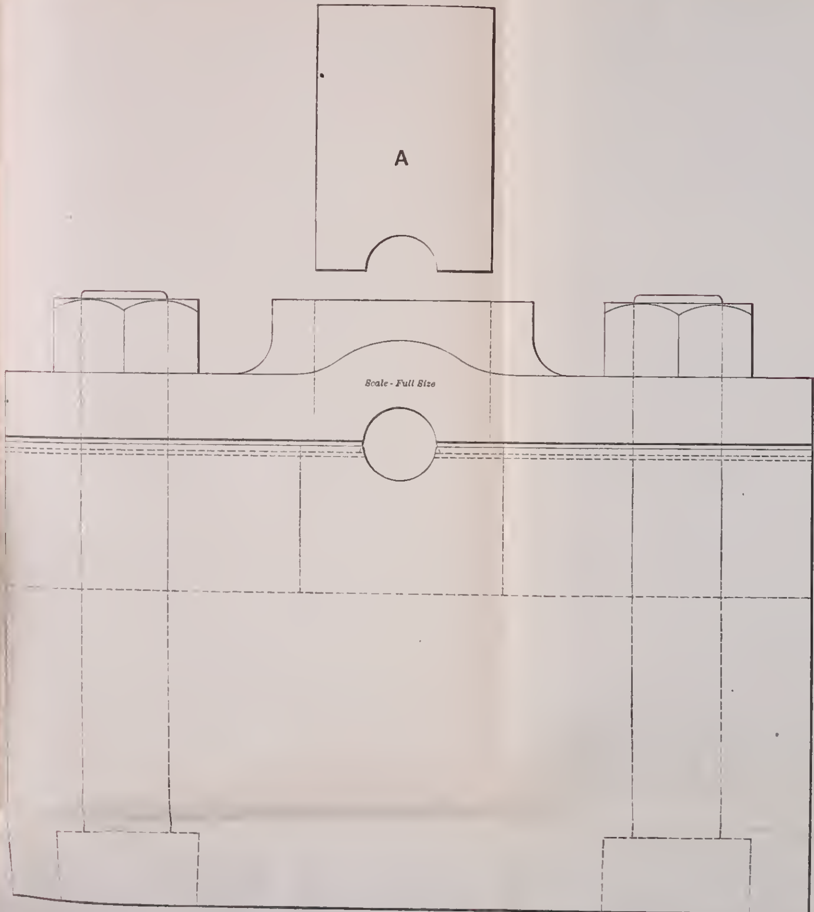
Dr. Arthur Tees' "Lilliput Furnace" was on exhibition, intended for the fusion of dental porcelain and the manufacture of continuous gum dentures. This denture consists of a metallic base of pure platina, to which is soldered with gold the moulded porcelain teeth. Around this is baked a mineral compound, allica in composition, to the body of the tooth, but fusing at a lower temperature, with a thin coating of enamel over it. The furnace is the result of experiment to get the requisite amount of heat for fusing the compound with the smallest possible amount of fuel. It is made of fire clay, strongly bound with iron, and is 15 inches high, 12 inches wide, and 8 inches deep, with 1-inch walls. Like all these furnaces, it is divided into three sections, for convenience of handling, cleaning and making the fire. Both the upper and middle sections have hearths, and the lower section a cast iron grate instead of movable bars, thus obviating the wearing of the fire clay. This furnace holds less than a peck of coke, and requires but thirty minutes to reach an intense heat, which will last that much longer. From the rectangular form of the furnace the heat strikes the whole surface of the muffle more evenly, it is claimed, than in other forms.

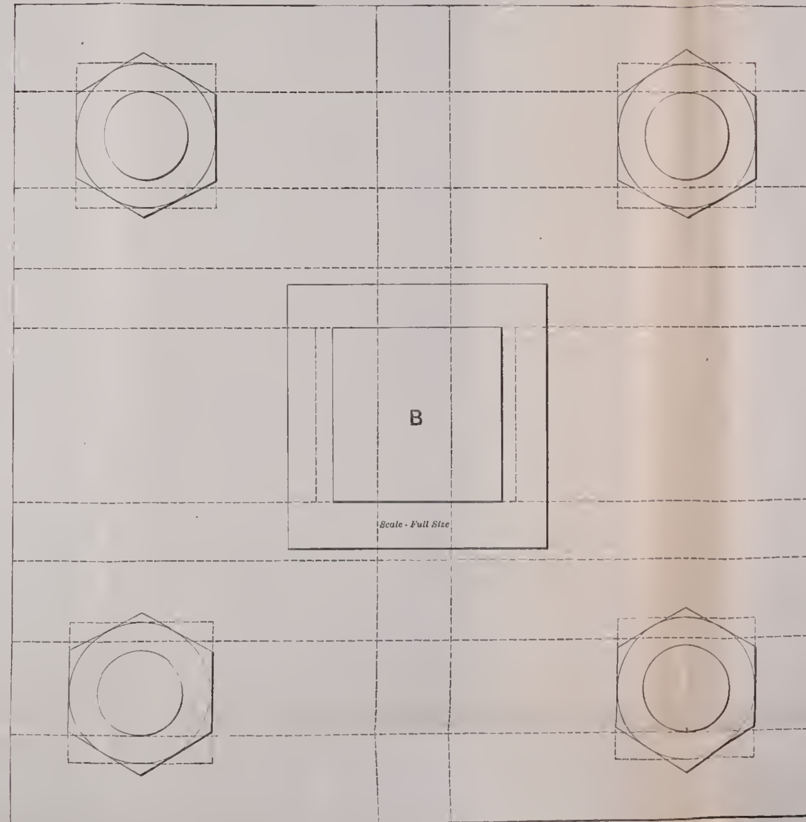
A very ingenious model of a universal shaft coupling, the invention of Mr. Deschamps, was also exhibited.

On motion, the Institute adjourned.

ISAAC NORRIS, M.D., *Secretary.*

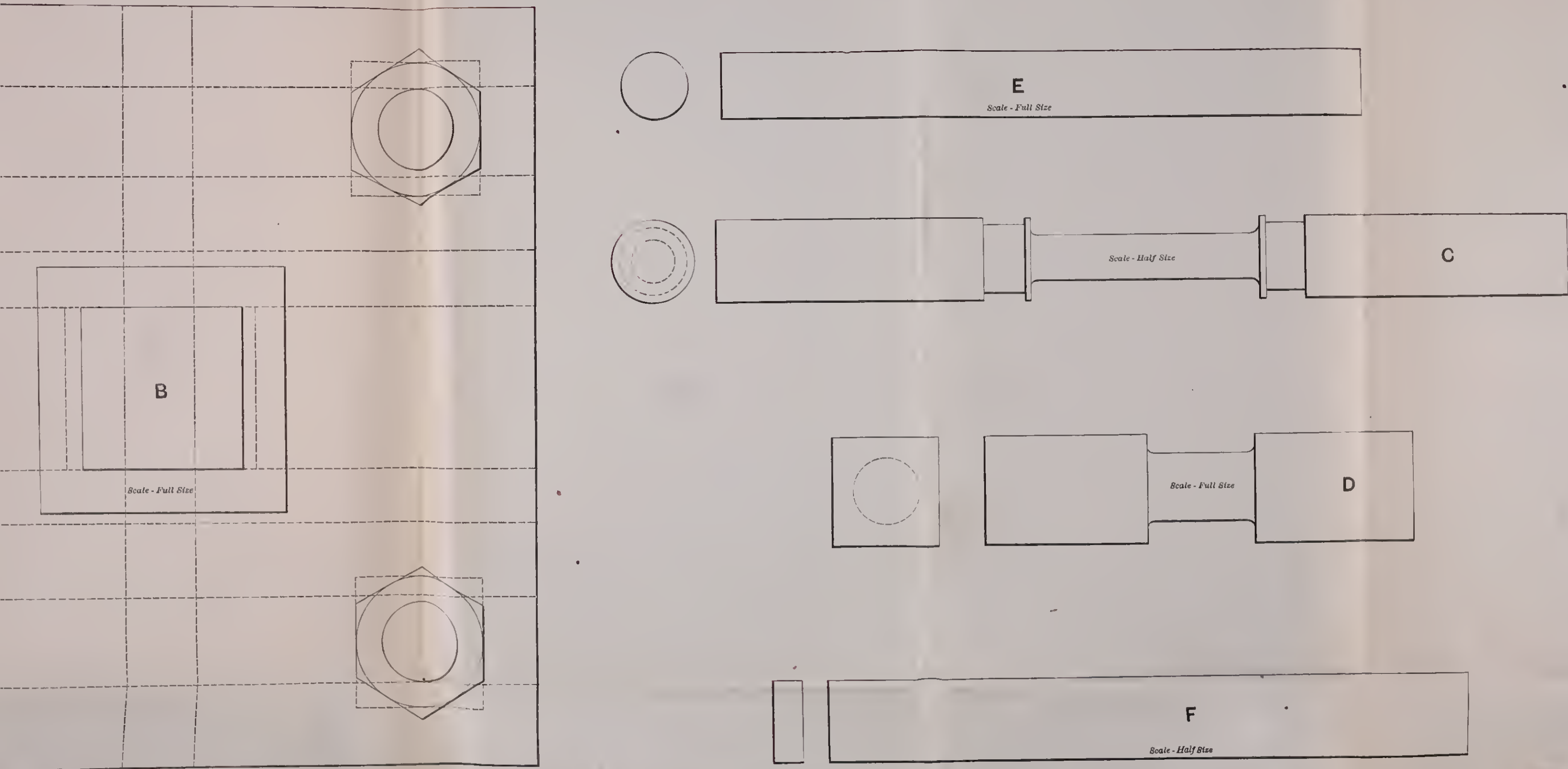
Shearing Apparatus and Test Pieces.



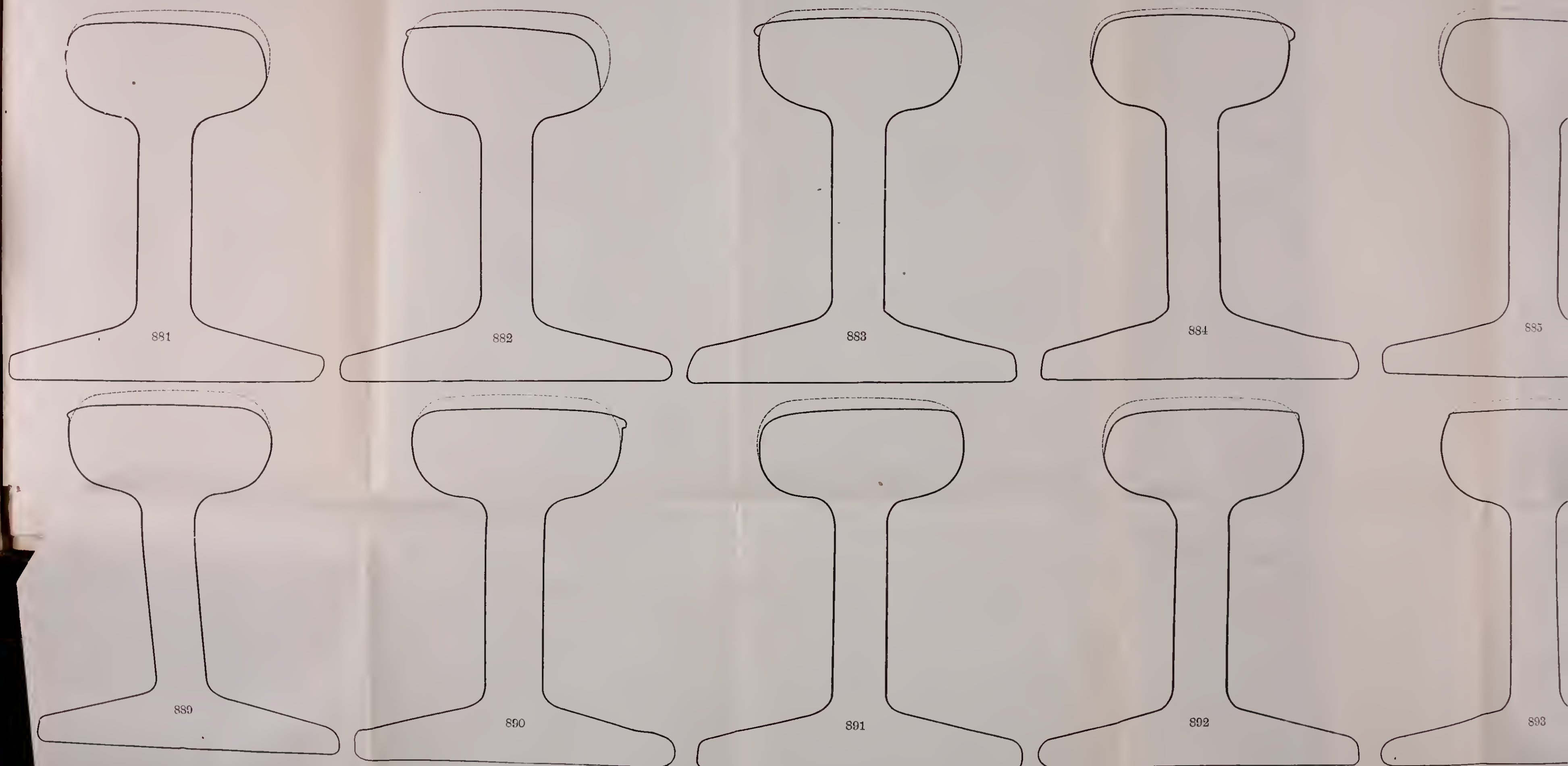


hearing Apparatus and Test Pieces.

Plate 1.

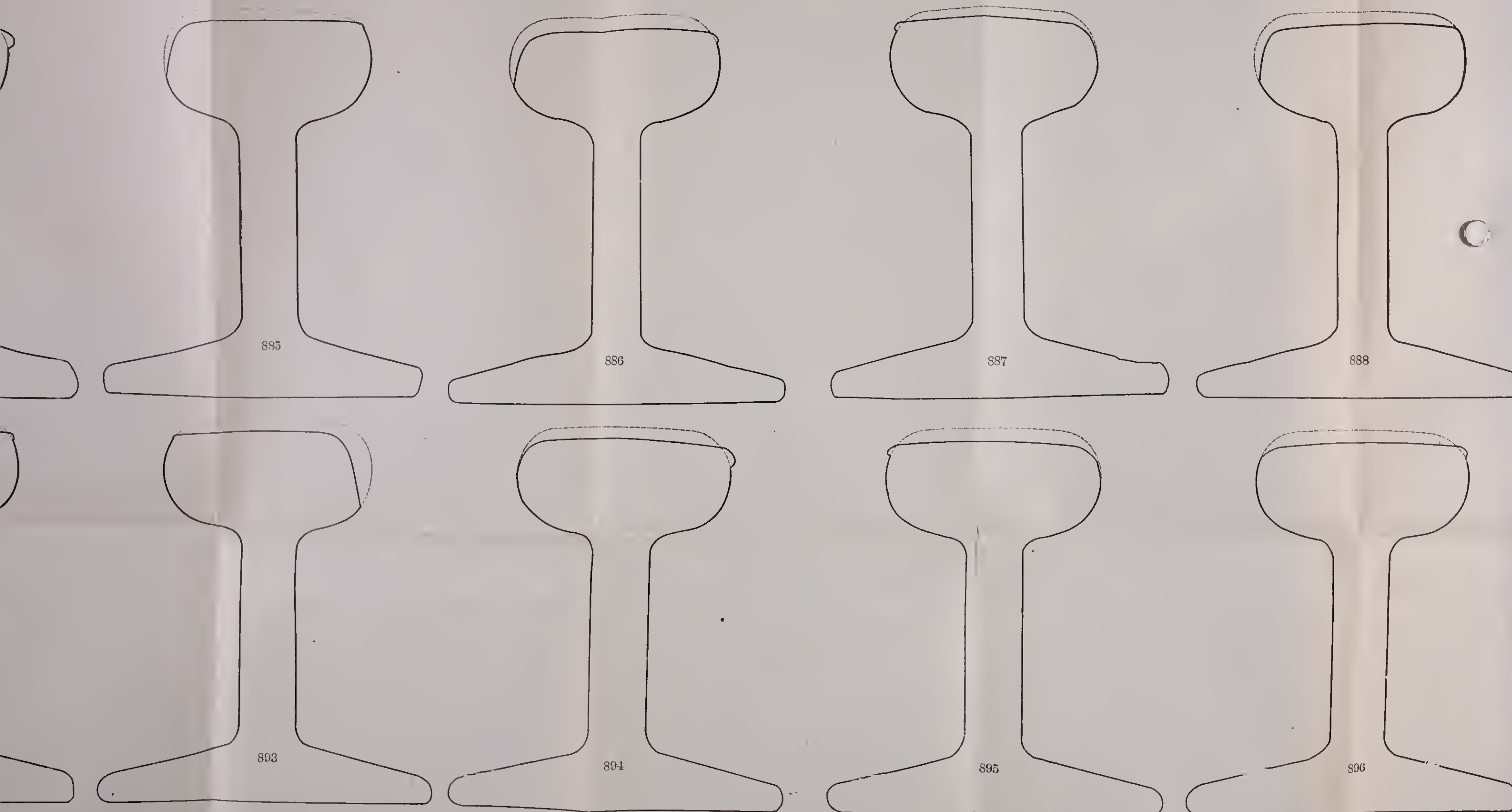


Diagrams of Rails on Grade Tangents.



of Rails on Grade Tangents.

Plate 2.



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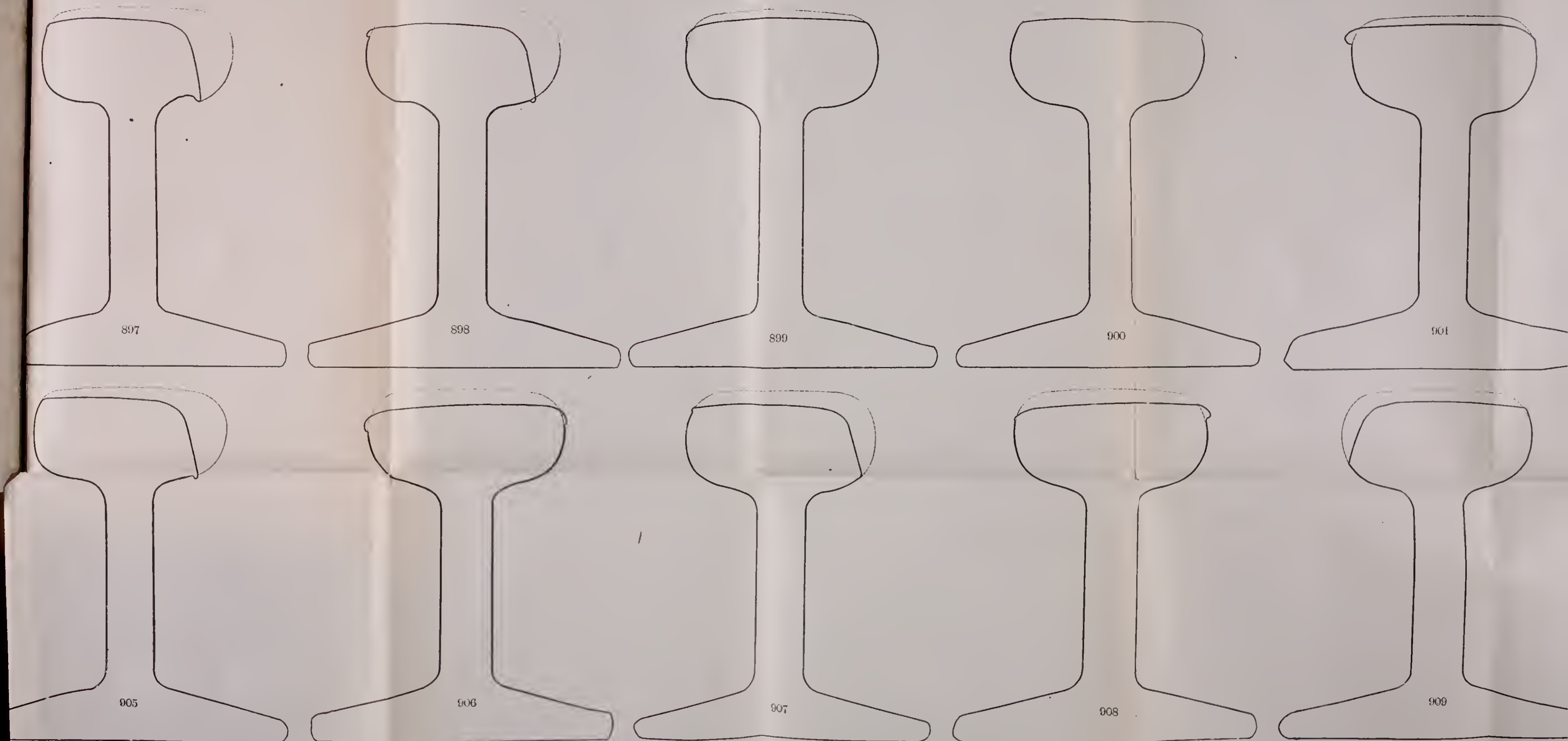


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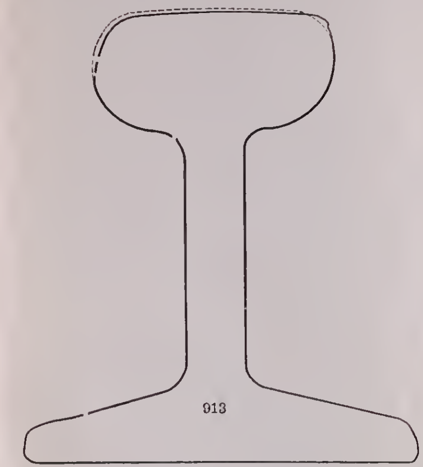
Diagrams of Rails on Level Tangents.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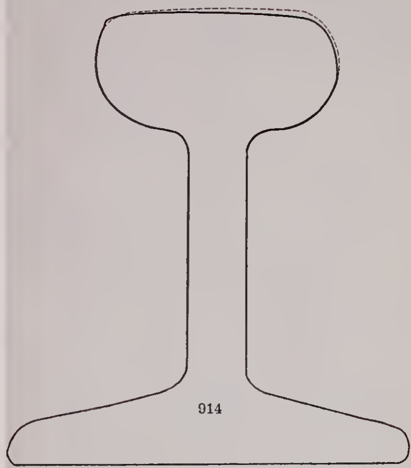
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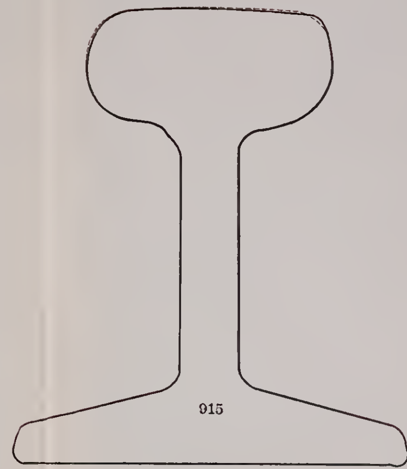
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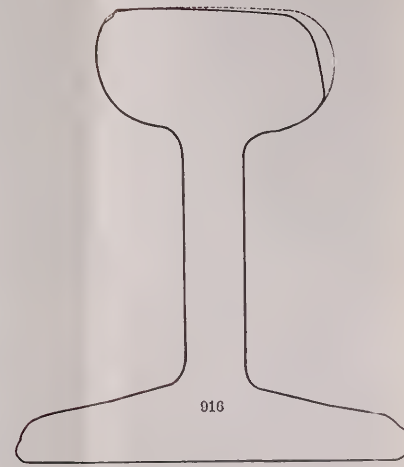
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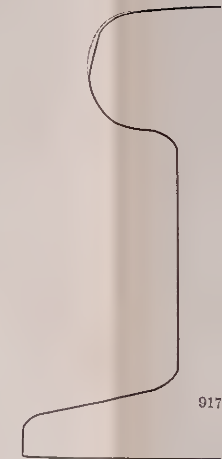
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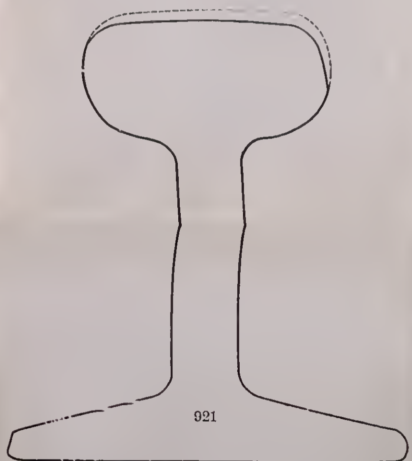
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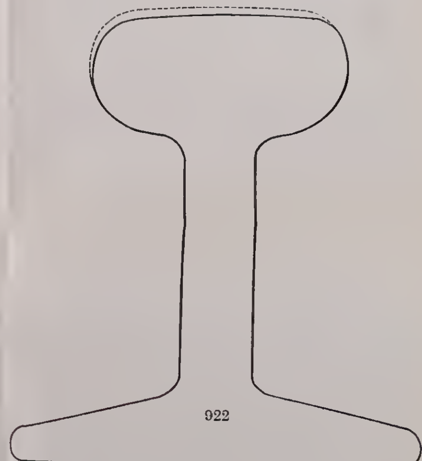
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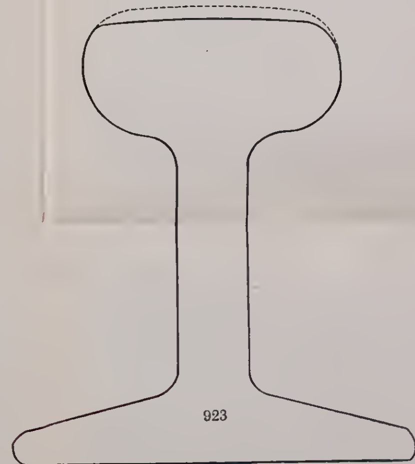
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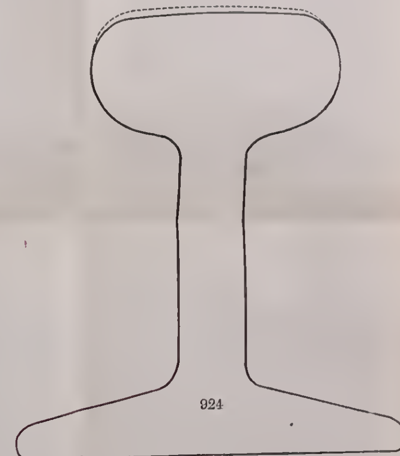
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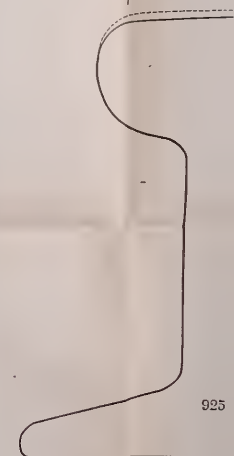
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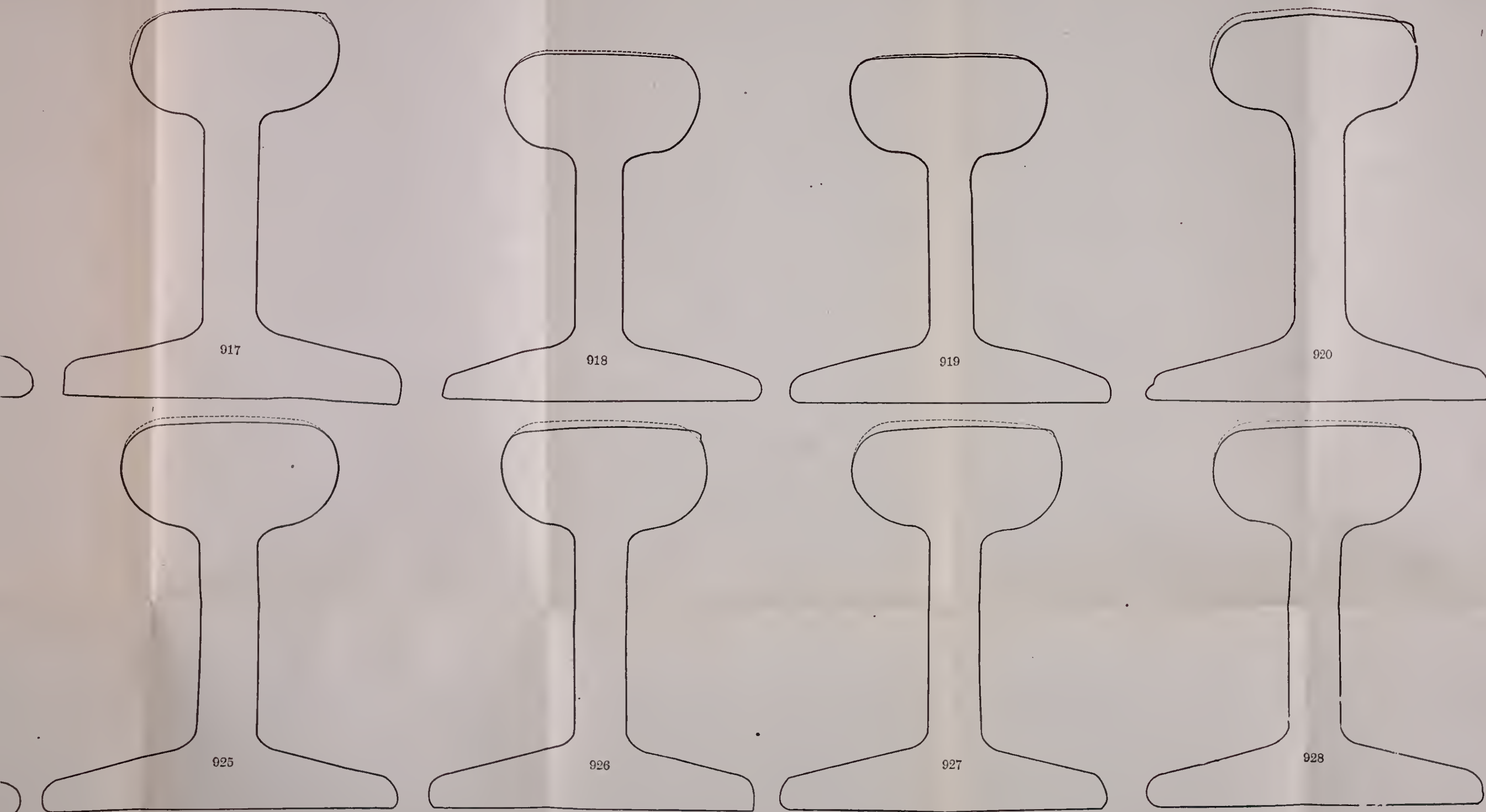
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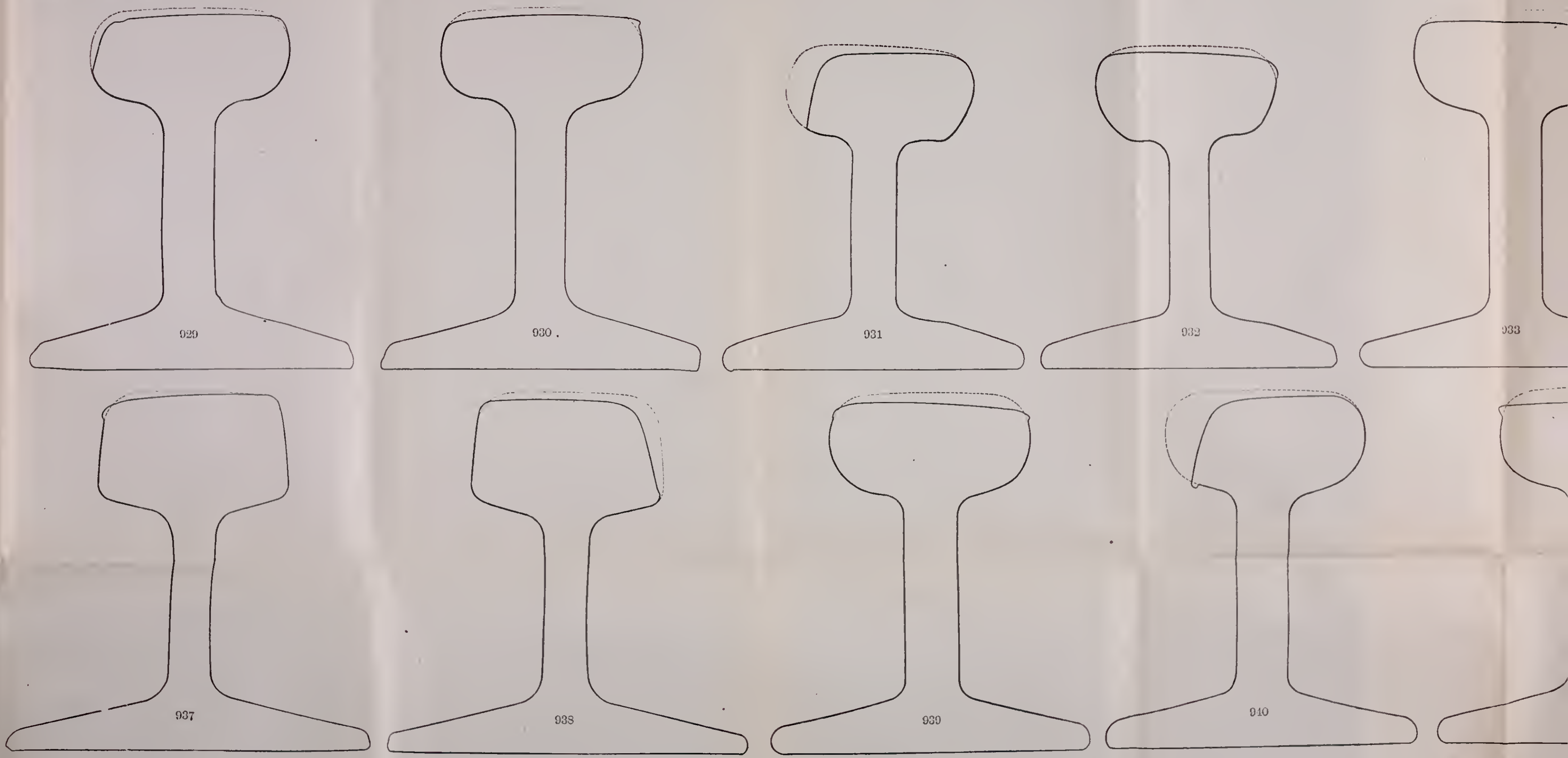
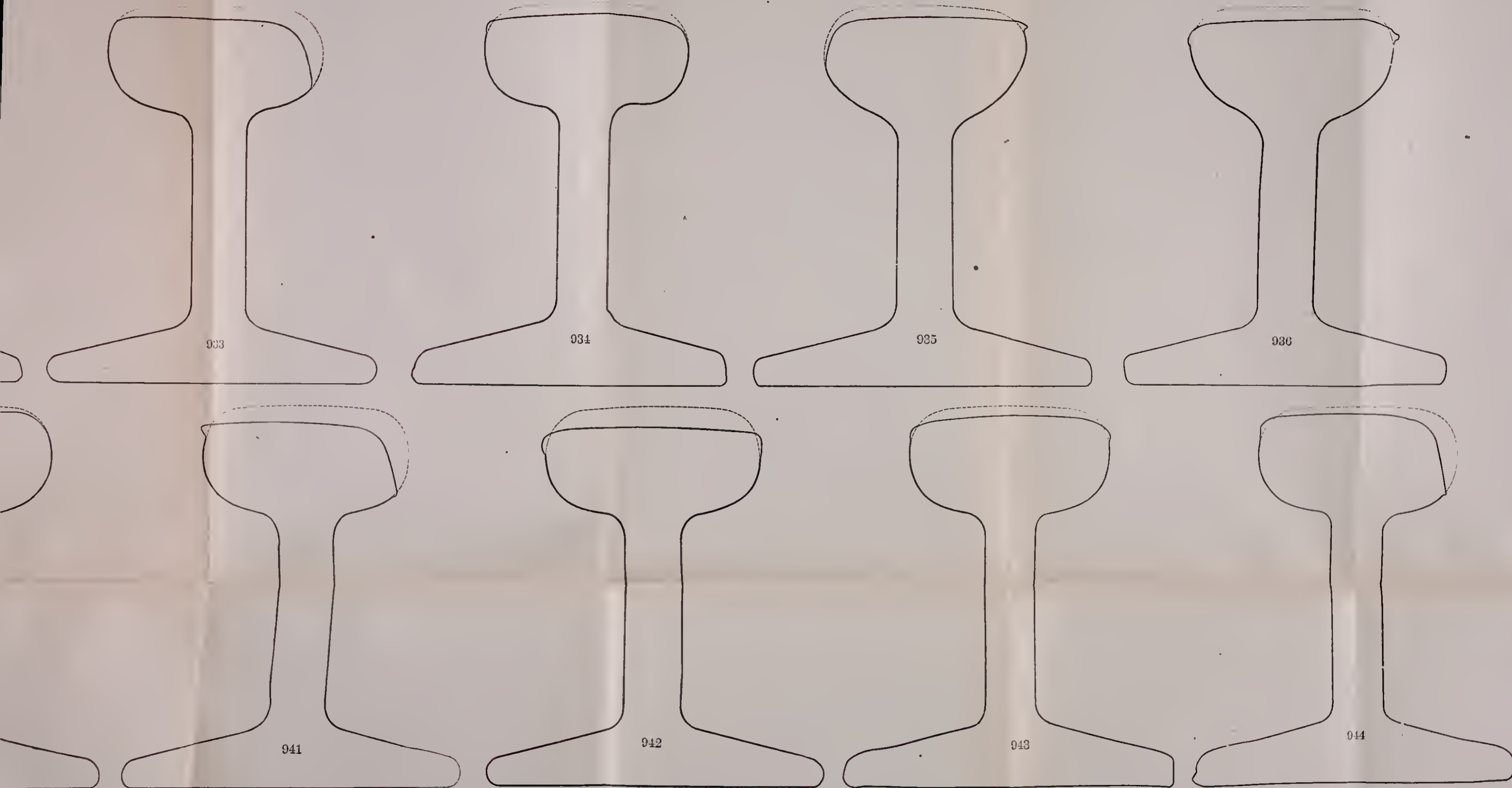
Diagrams of Rails on Level Curves.

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Service.	No.	Time of service.	Track.	Degree of curve.	Grade.	Weights per yard. Original.	Weights per yard. Present.	Weights per yard. Loss.	Tonnage.	Loss per million tons.	Torsion tests. Height of diagram.	Torsion tests. Length of diagram.	Torsion tests. Elastic limit.	Torsion tests. Area of diagram.	Tensile tests. Unannealed. Tensile strength.	Tensile tests. Unannealed. Elastic limit.	Tensile tests. Unannealed. Elongation.	Tensile tests. Annealed. Tensile strength.	Tensile tests. Annealed. Elastic limit.	Tensile tests. Annealed. Elongation.	Shearing tests. Shearing stress.	Shearing tests. Detrusion.	Bending tests. Maximum load.	Bending tests. Deflection.	Chemical analyses. Carbon.	Chemical analyses. Phosphorus.	Chemical analyses. Silicon	Chemical analyses. Manganese.	Phosphorus units.	Density.
Tangent, grade, against traffic.	887	11 yrs.	North.		89.76	66.79	64.66	2.13	55,197,994	.0386	2.78	18.13	0.95	41.85	69,000	34,000	25	67,000	28,000	24	49,000	.1000	2770	190°	.287	.048	.023	.435	24.2	.28143
	884	10 yrs. 2 mos.	"		95.04	68.73	66.60	2.13	52,174,969	.0408	3.03	14.73	1.03	36.37	75,000	36,000	24	74,000	33,000	22	55,000	.0875	3090	190°	.279	.082	.043	.656	32.7	.28214
	883	10 yrs. 2 mos.	"		95.04	67.87	65.43	2.54	52,174,969	.0487	3.20	10.89	1.16	28.79	77,000	36,000	4	86,000	36,000	12	62,700	.0925	3090	190°	.352	.104	.029	.610	35.7	.28214
	885	11 yrs.	"		89.76	68.21	65.47	2.74	55,197,994	.0496	3.24	13.30	1.19	36.02	84,000	39,000	20	81,000	37,000	21	57,000	.0950	3110	190°	.368	.057	.130	.472	33.9	.28143
	891	7 yrs. 1 mo.	South.		40.13	67.14	64.00	3.14	53,687,192	.0585	3.16	13.68	1.11	35.79	79,000	34,000	24	78,000	35,000	21	55,500	.1000	3140	70°	.305	.133	.022	.656	37.7	.28157
	888	11 yrs.	North.		89.76	64.22	60.78	3.44	55,197,994	.0623	2.79	21.74	1.10	51.00	66,000	33,000	24	65,000	28,000	25	50,000	.1150	2670	190°	.208	.038	.074	.344	21.3	.28129
	892	7 yrs. 1 mo.	South.		40.13	68.15	64.69	3.46	53,687,192	.0644	3.24	15.94	1.10	43.13	79,000	33,000	21	77,000	37,000	21	58,000	.0825	3230	72½°	.307	.114	.018	.542	33.7	.28229
	881	11 yrs. 1 mo.	North.		92.40	69.03	65.18	3.85	55,546,811	.0693	4.00	10.95	1.54	37.75	108,000	56,000	15	98,000	52,000	18	68,500	.0950	4260	86°	.483	.035	.480	.782	57.2	.28143
	894	5 yrs. 2 mos.	South.		52.80	67.47	64.54	2.93	38,088,574	.0769	3.55	8.46	1.37	25.96	93,000	45,000	5	82,000	44,000	6	70,000	.0650	3800	139°	.432	.118	.043	.880	45.9	.28086
	889	5 yrs. 11 mos.	"		21.12	68.56	65.02	3.54	44,620,100	.0793	3 05	14.48	1.17	37.92	76,000	36,000	21	75,000	40,000	22	56,000	.0875	3030	137°	.298	.085	.067	.568	33.1	.28114
	895	5 yrs. 2 mos.	"		52.80	67.92	64.89	3.03	38,088,572	.0796	3.39	5.47	1.48	15.42	60,000	30,000	1	56,000	40,000	1	64,500	.0675	3610	118½°	.470	.111	.063	.784	45.5	.28057
	896	5 yrs. 2 mos.	"		52.80	68.46	65.42	3.04	38,088,572	.0798	3.61	12.05	1.76	36.79	93,000	40,000	20	89,000	40,000	17	63,000	.0800	3620	35°	.415	.097	.063	.694	40.5	.28200
	886	11 yrs.	North.		89.76	05.72	61.24	4.48	55,197,994	.0811	3.04	12.29	1.11	30.84	78,000	34,000	19	75,000	28,000	19	55,000	.0900	2790	190°	.319	.069	.026	.404	27.9	.28243
	890	5 yrs. 11 mos.	South.		21.12	69.79	66.15	3.64	44,620,100	.0816	3.37	12.65	1.20	36.77	85,000	36,000	21	83,000	36,000	19	60,500	.0750	3360	113°	.365	.126	.059	.702	41.7	.28100
	882	11 yrs. 1 mo.	North.		92.40	68.36	62.58	5.78	55,546,811	.1040	2.99	14.68	1.03	36.37	76,000	34,000	21	73,000	30,000	23	53,500	.0925	2870	179°	.320	.060	.055	.442	28.1	.28143
	893	5 yrs. 2 mos.	South.		21.12	68.45	64.39	4.06	38,088,572	.1066	3.37	12.92	1.14	37.15	89,000	38,000	17	86,000	39,000	11	61,500	.0825	3480	156°	.387	.091	.032	.893	41.1	.28214
Curve, grade, with traffic. Low side.	899	11 yrs. 2 mos.	North.	5	21.12	64.18	61.94	2.24	52,370,617	.0428	2.72	18.32	1.01	41.68	68,000	30,000	25	65,000	28,000	26	51,000	.0925	2620	190°	.263	.051	.038	.326	22.3	.28214
	900	11 yrs. 2 mos.	"	5	21.12	64.46	62.02	2.44	52,370,617	.0466	3.16	11.19	1.16	29.96	84,000	34,000	17	82,000	32,000	16	59,500	.0875	3080	184½°	.428	.030	.059	.598	32.2	.28243
	901	7 yrs. 11 mos.	"	5	39.60	68.26	66.03	2.23	40,061,230	.0556	2.72	15.73	1.03	36.01	68,000	32,000	24	67,000	32,000	24	51,500	.1000	2490	190°	.233	.083	.027	.372	24.9	.28229
	906	10 yrs. 10 mos.	"	5	47.52	67.68	63.82	3.86	50,648,939	.0762	2.94	17.41	1.05	41.96	71,000	33,000	24	69,000	25,000	22	51,500	.0875	2870	190°	.308	.053	.057	.462	27.5	.28214
	910	8 yrs.	South.	5	89.76	67.28	61.30	5.98	62,813,664	.0952	3.07	11.31	1.14	28.41	79,000	33,000	22	75,000	28,000	16	55,500	.0875	2660	190°	.343	.098	.020	.478	31.8	.28114
	904	2 yrs. 11 mos.	"	5	39.60	67.85	65.63	2.22	21,504,824	.1032	3.06	13.07	1.03	33.11	79,000	33,000	16	77,000	34,000	16	56,000	.0875	2810	190°	.361	.100	.035	.552	34.7	.28100
	911	2 yrs. 3 mos.	"	5	89.76	68.05	66.23	1.82	17,226,993	.1056	3.64	3.21	1.64	9.12	68,000	48,000	1	69,000	30,000	1	73,500	.0550	3760	140°	.618	.154	.039	1.044	58.8	.28143
	908	4 yrs. 2 mos.	"	4	95.04	68.19	64.44	3.75	32,428,614	.1156	3.25	9.43	1.19	25.88	81,000	39,000	7	75,000	30,000	5	60,000	.0850	3350	175½°	.430	.158	.032	.552	42.7	.28129
Curve, grade, with traffic. High side.	902	7 yrs. 11 mos.	North.	5	39.60	67.12	62.25	4.87	40,061,230	.1215	3.01	13.04	1.00	32.13	79,000	38,000	8	76,000	28,000	14	57,500	.0925	2640	111°	.322	.077	.026	.492	29.5	.28214
	905	10 yrs. 10 mos.	"	5	47.52	64.92	58.32	6.60	50,648,939	.1303	2.93	14.06	1.11	35.12	74,000	33,000	20	70,000	30,000	22	54,500	.0850	2710	174°	.342	.026	.044	.460	25.4	.28243
	909	4 yrs. 2 mos.	South.	4	95.04	67.72	63.47	4.25	32,428,614	.1310	2.52	3.56	1.62	9.99	76,000	42,000	2	78,000	44,000	2	73,500	.0700	3420	50°	.600	.145	.041	.680	42.1	.28086
	898	11 yrs. 2 mos.	North.	5	21.12	65.05	58.06	6.99	52,370,617	.1335	2.96	13.75	1.08	35.07	74,000	33,000	21	71,000	34,000	23	53,000	.0875	2670	170°	.358	.040	.028	.538	28.1	.28157
	897	11 yrs. 2 mos.	"	5	21.12	64.49	57.18	7.31	52,370,617	.1396	3.24	11.65	1.25	32.43	82,000	36,000	18	80,000	32,000	17	59,500	.0800	2800	170°	.431	.049	.046	.582	33.2	.28186
	912	6 yrs.	South.	5	95.04	68.29	59.78	8.51	47,438,145	.1794	3.45	11.74	1.31	33.98	91,000	38,000	10	88,000	50,000	12	61,500	.0750	3759	103°	.401	.083	.056	.816	40.8	.28157
	907	4 yrs. 10 mos.	North.	5	47.52	65.71	59.72	5.99	24,211,147	.2474	3.53	13.04	1.33	38.82	90,000	40,000	21	86,000	44,000	20	62,000	.0850	3580	101°	.350	.102	.124	.748	43.1	.28200
	903	2 yrs. 11 mos.	South.	5	39.60	68.28	61.39	6.89	21,504,824	.3204	2.57	7.36	1.04	16.10	71,000	34,000	9	69,000	34,000	12	55,500	.0975	2860	190°	.355	.108	.029	.490	33.8	.28114

Service.	No.	Time of service.	Track.	Degree of curve	Weights per yard. Original.	Weights per yard. Present.	Weights per yard. Loss	Tonnage.	Loss per million tons.	Torsion tests. Height of diagram.	Torsion tests. Length of diagram.	Torsion tests. Elastic limit.	Torsion tests. Area of diagram.	Tensile tests. Unannealed. Tensile strength.	Tensile tests. Unannealed. Elastic limit.	Tensile tests. Unannealed. Elongation.	Tensile tests. Annealed. Tensile strength.	Tensile tests. Annealed. Elastic limit.	Tensile tests. Annealed. Elongation.	Shearing tests. Shearing stress.	Shearing tests. Detrusion.	Bending tests. Maximum load.	Bending tests. Deflection.	Chemical analyses. Carbon.	Chemical analyses. Phosphorus.	Chemical analyses. Silicon.	Chemical analyses. Manganese.	Phosphorus units.	Density.
Level tangent.	917	6 yrs.	North.		67.14	66.94	0.20	31,127,829	.0064	3.59	11.86	1.45	36.34	93,000	42,000	19	89,000	45,000	18	65,000	.080	2930	13°	.423	.127	.083	.708	45.1	.28186
	919	10 yrs. 6 mos.	North.		55.61	55.10	0.51	51,720,011	.0098	2.72	17.22	1.07	39.20	64,000	32,000	24	62,000	30,000	25	51,500	.1025	2270	190°	.247	.041	.023	.344	20.3	.28243
	918	10 yrs. 6 mos.	North.		55.91	55.20	0.71	51,720,011	.0137	2.89	17.92	1.21	43.29	70,000	34,000	20	69,000	35,000	24	54,500	.095	2520	190°	.219	.061	.181	.376	29.9	.28200
	915	6 yrs. 1 mo.	North.		64.91	64.40	0.51	31,514,889	.0162	2.21	7.75	1.01	15.17	71,000	34,000	4	62,000	25,000	4	60,000	.0775	3030	184°	.368	.113	.026	.714	39.2	.28100
	926	9 yrs. 3 mos.	South.		64.96	63.43	1.53	76,409,123	.0200	3.05	16.97	1.22	42.56	73,000	40,000	26	71,000	36,000	24	55,500	.100	2680	190°	.211	.145	.033	.352	30.2	.28271
	913	9 yrs. 4 mos.	North.		65.52	64.51	1.01	45,855,101	.0220	3.13	12.18	1.16	32.20	72,000	39,000	7	76,000	35,000	18	58,000	.090	2630	190°	.335	.102	.036	.508	33.4	.28143
	927	9 yrs. 3 mos.	South.		64.98	63.25	1.73	76,409,123	.0226	3.07	19.01	1.55	48.91	72,000	40,000	26	71,000	35,000	24	58,500	.0975	2710	190°	.187	.131	.049	.320	28.1	.28214
	914	9 yrs. 4 mos.	North.		64.78	63.46	1.32	45,855,101	.0288	2.60	10.80	.97	23.28	64,000	32,000	14	64,000	30,000	17	52,000	.100	2530	190	.270	.108	.015	.322	26.9	.28157
	928	5 yrs.	South.		66.25	64.93	1.32	43,610,150	.0303	2.98	3.37	1.32	8.29	60,000	44,000	1	68,000	50,000	2	66,000	.080	3190	173°	.428	.142	.042	.564	41.9	.28214
	920	11 yrs. 7 mos.	North.		67.61	65.78	1.83	52,991,684	.0345	2.95	18.27	1.00	43.99	69,000	32,000	25	68,000	25,000	24	53,500	.100	2630	190°	.293	.063	.039	.326	24.6	.28200
	924	4 yrs. 1 mo.	South.		68.31	66.79	1.52	36,349,989	.0418	3.67	12.64	1.37	39.71	93,000	39,000	20	90,000	36,000	18	66,000	.100	3550	111	.388	.123	.059	.806	44.2	.28229
	916	6 yrs.	North.		67.25	65.83	1.42	31,127,829	.0456	3.18	10.67	1.25	28.47	83,000	39,000	17	81,000	40,000	18	60,500	.0875	3010	154½	.329	.132	.044	.554	37.5	.28171
	925	4 yrs. 1 mo.	South.		68.36	66.63	1.73	36,349,989	.0476	3.70	14.95	1.36	47.58	92,000	40,000	13	90,000	40,000	18	66,500	.095	3980	73°	.389	.109	.069	.824	13.8	.28243
	922	5 yrs. 4 mos.	North.		69.04	67.01	2.03	27,622,230	.0735	3.74	12.57	1.37	39.97	94,000	39,000	19	91,000	35,000	17	66,500	.090	3760	80°	.428	.109	.038	.870	44.7	.28243
	923	6 yrs. 1 mo.	North.		68.58	65.66	2.92	31,514,889	.0926	2.21	4.55	1.08	10.59	64,000	34,000	3	54,000	34,000	2	64,500	.085	3360	155	.452	.144	.037	.708	45.5	.27943
	921	5 yrs. 4 mos.	North.		67.23	64.18	3.05	27,622,230	.1104	3.34	11.68	1.20	33.19	84,000	37,000	18	82,000	35,000	16	65,000	.090	3310	172°	.340	.096	.042	.746	37.9	.28257
Level curve. Low side.	932	10 yrs. 6 mos.	North.	3	56.17	54.95	1.22	51,720,011	.0236	2.92	16.53	1.09	39.40	69,000	34,000	20	70,000	35,000	23	57,000	.100	2340	190°	.269	.047	.026	.372	22.4	.28286
	937	3 yrs. 4 mos.	South.	2	66.77	65.96	0.81	29,905,122	.0271	3.00	4.84	1.34	12.12	68,000	38,000	2	68,000	36,000	3	68,500	.085	3220	168°	.154	.145	.015	.726	44.8	.28129
	936	12 yrs. 9 mos.	South.	2	68.02	65.17	2.85	92,025,478	.0309	3.01	15.56	1.05	38.72	72,000	34,000	23	70,000	26,000	22	53,500	.1025	2790	190°	.353	.039	.035	.318	23.8	.28259
	930	11 yrs. 7 mos.	North.	2¼	66.38	64.65	1.73	52,991,684	.0327	3.03	14.77	1.04	36.94	73,000	31,000	23	70,000	25,000	22	53,500	.1000	2820	190	.394	.035	.038	.288	24.3	.28286
	934	9 yrs. 8 mos.	South.	3½	67.14	64.09	3.05	78,364,968	.0389	2.95	13.89	1.26	34.43	74,000	39,000	22	74,000	35,000	22	61,000	.1025	2950	190°	.187	.144	.015	.252	27.8	.28214
	943	6 yrs. 3 mos.	South.	2	66.84	64.30	2.54	55,127,464	.0461	3.04	16.05	1.10	41.08	74,000	34,000	24	71,000	38,000	24	55,500	.100	2780	159½	.314	.061	.025	.602	29.8	.28214
	939	4 yrs. 2 mos.	South.	4	68.11	65.27	2.84	37,150,179	.0764	3.76	7.63	1.47	24.51	90,000	40,000	3	95,000	41,000	6	72,000	.090	3350	20°	.579	.115	.036	.718	47.0	.28171
	942	4 yrs. 10 mos.	South.	3	67.93	62.67	5.26	42,277,638	.1244	3.52	5.38	1.60	15.48	90,000	48,000	2	64,000	40,000	1	64,500	.0575	3240	108½°	.497	.136	.062	.724	47.8	.28143
Level curve. High side.	935	12 yrs. 9 mos.	South.	2	68.19	64.93	3.26	92,025,478	.0354	3.30	11.80	1.14	32.70	86,000	39,000	10	82,000	30,000	20	64,000	.095	3130	142½°	.460	.045	.059	.406	30.2	.28259
	929	11 yrs. 7 mos.	North.	2¼	68.89	66.45	2.44	52,991,684	.0460	2.89	16.28	1.18	39.95	69,000	36,000	24	70,000	35,000	24	55,500	.1125	2750	190	.235	.055	.080	.300	23.3	.28259
	933	9 yrs. 8 mos.	South.	3½	67.61	63.15	4.46	78,364,968	.0569	3.16	8.19	1.20	21.91	82,000	38,000	5	67,000	35,000	3	65,000	.085	3180	186°	.376	.087	.054	.558	35.1	.28143
	944	6 yrs. 3 mos.	South.	2	66.72	63.47	3.25	55,127,464	.0589	3.00	6.78	1.24	16.68	75,000	34,000	4	78,000	33,000	8	61,500	.095	3040	105°	.441	.057	.035	.702	36.1	.28214
	931	10 yrs. 6 mos.	North.	3	57.09	52.61	4.48	51,720,011	.0866	2.81	17.70	1.10	42.24	66,000	34,000	24	64,000	27,000	24	52,000	.110	2430	190°	.260	.047	.029	.416	23.1	.28229
	938	3 yrs. 4 mos.	South.	2	68.70	64.85	3.85	29,905,122	.1287	3.06	12.16	1.02	29.53	79,000	36,000	16	77,000	35,000	16	59,500	.0925	2950	146	.343	.115	.035	.594	37.5	.28143
	941	4 yrs. 10 mos.	South.	3	66.92	60.83	6.09	42,277,638	.1440	3.43	13.88	1.25	40.30	87,000	39,000	19	83,000	37,000	16	63,500	.095	3290	112°	.404	.111	.065	.716	42.1	.28186
	940	4 yrs. 2 mos.	South.	4	68.17	61.77	6.40	37,150,179	.1723	3.80	5.38	1.62	16.73	76,000	52,000	1	80,000	44,000	2	77,500	.0725	3220	17½°	.548	.127	.046	.680	46.9	.28214

ANNEALED.

e h.	Elastic limit.
0	35,750
5	37,125
0	29,250
0	30,500
0	34,000
0	40,000
0	33,875
0	36,875
0	30,500
0	38,500
0	33,250
0	35,750
8	33,167
2	36,167
0	33,625
5	37,875
9	33,281
8	36,594

AVERAGES.

Number of line.	Number of rails in average.	DESCRIPTION OF AVERAGES.	Loss per million tons.	Grade.	Degree of curve.	TORSION TESTS.				TENSILE TESTS.						SHEARING TESTS.		BENDING TESTS.		CHEMICAL ANALYSES.					
										UNANNEALED.			ANNEALED.												
						Height of diagram.	Length of diagram.	Elastic limit.	Area of diagram.	Tensile strength.	Elastic limit.	Elongation.	Tensile strength.	Elastic limit.	Elongation.	Shearing stress.	Detrusion.	Maximum load.	Deflection.	Carbon.	Phosphorus.	Silicon.	Manganese.	Phosphorus units.	Density.
1	8	Grade tangent, slower wearing.	.0540	79.00		3.18	14.92	1.15	38.84	79,625	37,625	19.6	78,250	35,750	20.5	56,962	.0959	3014	147°	.324	.076	.102	.562	34.8	.28184
2	8	Grade tangent, faster wearing.	.0861	50.49		3.29	11.62	1.28	32.15	81,250	36,625	15.6	77,375	37,125	14.7	60,500	.0800	3320	133°	.379	.095	.051	.669	87.9	.28141
3	4	Grade curve, low side, slower wearing.	.0553	32.34	5°	2.89	15.66	1.06	37.40	72,750	32,250	22.5	70,750	29,250	22.	53,375	.0919	2765	188°	.308	.054	.045	.439	26.7	.28225
4	4	Grade curve, low side, faster wearing.	.1049	78.54	4¼°	3.25	9.25	1.25	24.13	76,750	38,250	11.5	74,000	30,500	9.5	61,250	.0787	3145	174°	.438	.127	.031	.656	42.0	.28121
5	4	Grade curve, high side, slower wearing.	.1291	50.82	4¾°	2.85	11.10	1.20	28.08	75,750	36,500	12.8	73,750	34,000	15.2	59,625	.0837	2860	126°	.405	.072	.035	.542	31.3	.28175
6	4	Grade curve, high side, faster wearing.	.2217	50.82	5°	3.20	10.95	1.23	30.33	83,500	37,000	14.5	80,750	40,000	15.2	59,625	.0844	3247	141°	.384	.085	.064	.659	37.7	.28164
7	8	Level tangent, slower wearing.	.0174			2.91	14.22	1.20	35.12	72,375	36,625	17.5	70,500	33,875	19.2	56,875	.0928	2662	167°	.282	.104	.056	.455	31.6	.28189
8	8	Level tangent, faster wearing.	.0595			3.22	11.09	1.24	31.47	79,875	38,000	14.5	78,000	36,875	14.4	63,562	.0909	3349	138°	.381	.115	.046	.675	40.0	.28187
9	4	Level curve, low side, slower wearing.	.0286	$2\frac{5}{16}$°		2.99	12.92	1.13	31.79	70,500	34,250	17.	69,500	30,500	17.5	58,125	.0969	2792	184°	.367	.066	.028	.426	28.8	.28240
10	4	Level curve, low side, faster wearing.	.0715		3⅛°	3.32	10.74	1.36	28.87	82,000	40,250	12.7	76,000	38,500	13.2	63,250	.0875	3080	119°	.394	.114	.042	.574	38.1	.28185
11	4	Level curve, high side, slower wearing.	.0493		$2\frac{7}{16}$°	3.09	10.76	1.19	27.81	78,000	36,750	10.7	74,250	33,250	13.7	61,506	.0969	3025	150°	.378	.061	.057	.491	31.2	.28219
12	4	Level curve, high side, faster wearing.	.1329		3°	3.28	12.28	1.25	32.20	77,000	40,250	15.	76,000	35,750	14.5	63,125	.0925	2972	116°	.388	.100	.044	.601	37.4	.28193
13	24	Tangents and low sides curves, slower wearing.	.0378			3.01	14.48	1.15	36.19	74,542	35,833	18.9	72,958	33,167	19.8	56,529	.0943	2818	167°	.314	.080	.065	.483	31.4	.28201
14	24	Tangents and low sides curves, faster wearing.	.0779			3.27	10.90	1.28	30.04	80,167	37,958	14.1	76,792	36,167	13.5	62,104	.0847	3260	140°	.392	.110	.044	.653	39.3	.28160
15	8	High sides of curves, slower wearing.	.0892			2.97	10 93	1.19	27.94	76,875	36,625	11.7	74,000	33,625	14.4	60,562	.0903	2942	138°	.391	.066	.046	.516	31.2	.28197
16	8	High sides of curves, faster wearing.	.1773			3.24	11.61	1.24	31.27	80,250	38,625	14.7	78,375	37,875	14.8	61,375	.0884	3109	128°	.386	.092	.054	.630	37.5	.28178
17	32	All conditions, slower wearing.	.0506			3.00	13.59	1.16	34.13	75,125	36,031	17.1	73,219	33,281	18.5	57,537	.0933	2878	160°	.334	.077	.060	.491	31.3	.28201
18	32	All conditions, faster wearing.	.1028			3.26	11.08	1.27	30.35	80,188	38,125	14.2	77,188	36,594	13.8	61,922	.0856	3222	133°	.390	.106	.047	.647	38.9	.28165

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THE WEARING POWER OF STEEL RAILS IN RELATION TO THEIR CHEMICAL COMPOSITION AND PHYSICAL PROPERTIES.

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(Continued from page 196.)

Turning our attention now again to the tables before referred to, I think that, notwithstanding we do not find absolute uniformity, as has just been explained, yet no one can fail to observe, in general, a difference, both in physical properties and in chemical composition, between the slower-wearing and the more rapid-wearing rails. Thus, in regard to the physical properties, I think it is entirely evident to inspection, that, in the torsion tests, the slower-wearing rails in each group, except perhaps on the high sides of curves, are characterized in general by lower height and greater length of diagram. In the tensile tests, again, the slower-wearing rails are, in general, characterized by lower tensile strength and greater elongation than the more rapid-wearing ones. In the shearing tests, the same thing appears, viz., in general, lower shearing stress and greater detrusion. And in the bending tests we see the same result, perhaps more strongly than in

any of the other tests, viz., that the slower-wearing rails are, in general, those which have the lower bending stress and the greater amount of deflection before rupture. In density, as would be expected, the slower-wearing rails have the greater density. Again, in the chemical composition, the same thing is observable, viz., that the slower-wearing rails are characterized in general by the lower amounts of the substances determined, carbon, phosphorus, silicon, and manganese, while in phosphorus units, which, as you remember, I suggested as an attempt to measure, in a degree at least, the combined influence of these substances on the steel, the same thing may be seen, viz., in general, the slower-wearing rails are characterized by the lower numbers of phosphorus units.

Now, how shall we render evident to ourselves, in a more tangible manner, what seems, on inspection, to be the general teaching of the results we are studying. I know of no method except that of combining these results together and forming averages. This, I believe, is the universal method applicable to all cases where it is desired to draw conclusions from results embracing a number of samples. If, therefore, we make a series of averages of the results obtained, the individual peculiarities of the different rails will, to a certain extent, disappear, and the general conclusions become more evident.

Accompanying Plate 8 gives a number of such averages. The first line on this plate gives the averages obtained from combining together all the essential data of the eight slower-wearing rails of the first group. The second line gives the same data for the eight more rapid-wearing rails of the same group. The next ten lines give the corresponding averages obtained from the remaining five groups. Turning our attention now to these averages, I think the same general conclusions which we have already obtained become still more clearly evident, viz., that, perhaps, with the exception of the rails on the high sides of curves, the slower-wearing rails in each group are in general characterized by those peculiarities which we are accustomed to comprehend under the general term softer steel; that is, in general, diagrams of less height and greater length, lower tensile strength and greater elongation, lower shearing stress and greater detrusion, lower bending stress and greater deflection, and lower amounts of carbon, phosphorus, silicon, and manganese, and lower phosphorus units. The exceptions to the law, if we may so call it, will be readily noticed, and are, perhaps, inseparable from averages formed from so small a number

of samples as are some of these. The rails on the high sides of curves will be noticed further on.

With regard to the averages under consideration, it is perhaps interesting to notice that the slower-wearing rails are characterized throughout the whole series by lower height of diagram and lower elastic limit in the torsion tests, by lower elastic limit in the annealed tensile tests, by lower bending stress and greater deflection in the bending tests, by lower phosphorus and lower manganese, in the chemical analyses, by lower phosphorus units and by greater density. It should be remarked that the high average silicon of the slower-wearing rails on grade tangents is caused almost entirely by rail No. 881, a rail whose chemical composition and physical properties are certainly anomalous. This rail has 0·48 of silicon together with high carbon and manganese, and yet it gives with high tensile strength, as would be expected, a very good elongation. I may add that three determinations of the silicon in this rail were made, in order to be sure that there was no error in the work. The peculiarities of this rail cause the average of which it forms a part to differ from the other averages of the slower-wearing rails in several particulars. If this rail is thrown out, and an average made from seven rails instead of eight, all the data fall more nearly into line with the averages of the slower-wearing rails in the other groups. Thus, to give two or three examples, the average tensile strength of the unannealed tests becomes 75,143 lbs. instead of 79,625 lbs.; the elastic limit of same tests, 35,000 lbs. instead of 37,625 lbs.; the tensile strength of the annealed tests, 75,428 lbs. instead of 78,250 lbs.; the carbon, 0·301 instead of 0·324; the phosphorus, 0·082 instead of 0·076; the silicon, 0·048 instead of 0·102; the manganese, 0·530 instead of 0·563; and the phosphorus units, 31·3 instead of 34·8

With regard to the rails on the high sides of curves, it will be observed that they do not seem to be governed throughout by the same law as the remaining groups. Whatever may be the reason for this no one can fail to observe indications, which seem to point in the direction, that on the high sides of curves the harder rails give the better wear. This is especially noticeable in the group of rails on the high sides of level curves. It is, nevertheless, to be observed that in some of the data, especially height of diagram and elastic limit in the torsion tests, elastic limit in both the annealed and unannealed tensile tests, in the shearing tests, in the deflection of the bending tests, and

in the chemical composition the groups of rails on the high sides of curves do not differ from the remaining groups. It would not be surprising if more extended data should show that a different quality of steel would give better wear on the high sides of curves than was found best in the other conditions of service. For on tangents, and on the low sides of curves, the kind of wear to which the rails are subjected seems to be different in its nature from the wear on the high sides of curves. On tangents, and on the low sides of curves, the loss of metal is apparently due to two causes: 1st and principally, to rolling friction; and 2d, to the sliding of the wheels. Now on the high sides of curves, in addition to the loss of metal due to these causes, there is also that due to the flanges of the wheels, or flange wear. Now flange wear may not only be different in its nature, but may also require different qualities in steel to most successfully resist it from what are found best where the rails are subjected principally to rolling friction. This point will be referred to again when we come to speak of the nature of wear.

Thus far we have confined our study to the consideration of averages obtained from the rails within each group. But, if I am right, the value of averages increases with the number of samples from which they are derived. It will be instructive, therefore, to make averages of the slower and faster-wearing rails, embracing as large a number of samples as possible. But, as has just been noticed, the kind of wear to which rails have been subjected, whether rolling and sliding friction or flange wear, may have an influence. It will, therefore, perhaps be most instructive to make averages of all the rails which have been subjected to the same kind of wear, and see what their testimony is as to the physical properties and chemical composition of the slower-wearing rails. Now, forty-eight rails out of the sixty-four we are studying did their service under similar circumstances as to kind of wear; that is, forty-eight of the rails were on tangents and on the low sides of curves, and consequently had little or no flange wear, while sixteen of the rails were on the high sides of curves, and were consequently subjected to both kinds of wear.

In accompanying Plate 8, line No. 13 gives the averages of the twenty-four slower-wearing rails which did their service on tangents and on the low sides of curves, and were consequently subjected to the same kind of wear; while line No. 14 gives the averages of the twenty-four faster-wearing rails that did their service under the same con-

ditions. Line No. 15 gives the averages of the eight slower-wearing rails that were on the high sides of curves, and line No. 16 the averages of the eight faster-wearing rails under the same conditions. What, now, do these four lines of results teach?

With regard to the first two of these four lines, inspection seems to me to show very clearly that the slower-wearing rails are characterized by those physical properties and that chemical composition which we are accustomed to comprehend under the term softer steel. In other words, we find that the slower-wearing rails are characterized by lower height and greater length of diagram and lower elastic limit in the torsion tests; by lower tensile strength, lower elastic limit and greater elongation in the tensile tests; by lower shearing stress and greater detrusion in the shearing tests; by lower bending stress and greater deflection in the bending tests; by lower carbon, phosphorus and manganese, with a trifle higher silicon (caused by rail No. 881, as has been before explained) in the chemical composition; by lower phosphorus units, and by greater density.

With regard to the next two lines, giving the averages of the rails on the high sides of curves, the testimony of the data is not uniform. The length and area of diagram in the torsion tests, and the elongation of the tensile tests, and also in a slight degree the carbon, indicate that the slower-wearing rails are the harder steel. In every other item of the data, however, the slower-wearing rails are characterized by the same peculiarities that we have noticed in regard to the other rails, viz., lower height of diagram and lower elastic limit in the torsion tests; lower tensile strength and lower elastic limit in the tensile tests; lower shearing stress and greater detrusion in the shearing tests; lower bending stress and greater deflection in the bending tests; lower phosphorus, silicon and manganese, with lower phosphorus units in the chemical composition, and greater density. It may be that this want of uniformity in the data from the rails on the high sides of curves is due to the fact that our averages only embrace sixteen samples, and it may be, on the other hand, that an indefinite number of samples would give the same results. Or, again, it may be that this want of uniformity is due to a fact that has already been stated, viz., that on the high sides of curves the loss of metal is due to a mixed wear, partly from rolling and sliding friction and partly from flange wear. If a certain quality of steel most successfully resists loss of metal due to rolling and sliding friction, and a certain other quality of steel best

resists flange wear, then certainly it is not strange that in results obtained from a series of rails, which have been subjected to both these kinds of wear at once, there should not be absolute uniformity. If in the case of every rail the loss of metal was due almost entirely to rolling and sliding friction, or, on the other hand, if the loss of metal was due almost entirely to flange wear, there is very little doubt but that the law would appear. But it is not difficult to conceive that there may be in any series of rails such a relation between the loss of metal due to both of these causes as to make it quite difficult to discover what kind of steel is best adapted to the high sides of curves.

Nevertheless, in an investigation of the extent of the one under consideration, perhaps the best that we can expect to do is to get indications, and as far as may be definite conclusions, as to the kind of rail best adapted to resist wear on the road taken as a whole. It will, perhaps, therefore, not be wise to discuss further the peculiarities of steel rails best adapted to any one kind of service, but to ask: What is the teaching of all the results we have obtained? This teaching will of course become evident if we make two averages, the one of the slower-wearing and the other of the faster-wearing rails, the two embracing the whole sixty-four rails.

In accompanying Plate 8, lines Nos. 17 and 18 give such averages. An inspection now of these two lines seems to me to present in a very striking light the differences in the results of the physical tests and in the chemical composition, which are characteristic of the slower-wearing and the more rapid-wearing rails. In every particular of physical test and of chemical composition, except a trifle higher silicon, which, as has been before explained, is caused by rail No. 881, the averages of the 32 slower-wearing rails are such as belong to what we are accustomed to call softer steel. In other words, the slower-wearing rails have on the average less height and greater length of diagram, with lower elastic limit in the torsion tests; lower tensile strength, lower elastic limit and greater elongation in the tensile tests; lower shearing stress and greater detrusion in the shearing tests; lower bending stress and greater deflection in the bending tests; lower carbon, phosphorus, manganese and phosphorus units in the chemical composition, and greater density. It seems to me, therefore, that so far as we may trust the teachings of 64 rails, all of which have actually done service, we can hardly escape the conclusion that the softer rails give the better wear.

In view of this conclusion, two questions very naturally arise:

1st. If the softer rails give the better wear why not make rails even softer than those in the series we are now considering; or, in other words, what is the limit of softness to which it is possible to go?

And, 2d. With our present knowledge of steel, is there any conception which will at all assist us in understanding why it is that the softer rails give the better wear?

As to the first question, I think almost every practical man will say at once: If your conclusions are correct that the softer rails give the better wear, why not make rails as soft as possible? indeed, why not use iron rails instead of steel? In answer to this latter question I think it may be fairly said, that such metal as is used for rails which we are accustomed to call iron is quite a different metal from that commonly called steel which is used for rails. From the processes used in their manufacture, iron rails are never free from slag, and lack homogeneity of structure, while steel rails, from their method of manufacture, are almost entirely free from slag, and are, or should be, practically homogeneous throughout. In the puddling process by which the iron for iron rails is made, the metal when taken from the puddling furnace is a dripping mass of iron and slag, and the amount of slag that is removed is dependent, in great part at least, on the subsequent working. Furthermore, in the subsequent operations before the rail is finished there are plenty of chances for non-welding, so that an iron rail might almost be cited as an example of non-homogeneity of structure. In the processes by which steel rails are made, on the other hand, the metal previous to the formation of the ingot is in a state of fusion, thus allowing the almost complete separation of the slag, and the formation of ingots, from which rails are to be rolled, that are, or should be, practically homogeneous in structure. Now that rails made from ingot iron, if such a metal is possible, that is iron which before it became an ingot was in a state of fusion, would not, in actual service, wear slower and lose less metal per million tons' burden that passed over them than even the best rails in the series we are studying, is an assertion which I think few men would be willing to make, and fewer still would be able to maintain.

But, it seems to me, that there is a natural limit to the softness of steel for use in rails, which limit is determined by the service to which the rails are to be subjected. It is evident, I am sure, that besides their ability to successfully resist wear, and to withstand the shocks

and blows to which they may be subjected, rails have also another very important duty, viz., they must be able to hold up the load which is to pass over them without being deformed or squeezed out of shape by this load. They must also have sufficient stiffness so as not to be bent down between the ties by the load, and thus give rise to a series of elevations and depressions in the track. To take an extreme case for an example, if we had a rail made of lead no one doubts but that it would entirely fail to hold up the load between the ties, and would on the ties be deformed and squeezed out of shape, probably, by the first train that passed over it. Now those qualities of metal which enable it to hold up a load without bending, and to successfully resist deformation from compression, are generally comprehended with others under the term hardness. In other words, to apply this to the case we have in point, the harder the steel in the rails the less liable will they be to bend between the ties and to suffer deformation, or to squeeze out from the loads which are above them. Here, then, is the limit of softness. The steel in the rails must not be so soft that they will bend between the ties, or squeeze out of shape and become deformed from the compression due to the superincumbent load.

Just exactly where this limit shall be placed is of course a question to be decided by the weight of the locomotives and cars which are intended to run over the rail. On roads where the locomotives and cars are comparatively light the rails could be made softer, and consequently, if our conclusions are correct, the loss of metal per million tons' burden would be less than on roads using heavier locomotives and freighting their cars with heavier loads. Just where this limit of softness is in the case of the Pennsylvania railroad, it is of course impossible to say, but from an examination of all the rails which have been tested at Altoona, amounting to nearly a hundred samples, I think that steel which gives a tensile strength of 65,000 lbs. per square inch or over will give very little or no trouble from bending or deformation in the track. It may be that this limit might be still lower, but with our present knowledge of the subject, and without further experimentation, I should hardly like to recommend it. Another consideration comes in here, and that is, it is a little more difficult to successfully make soft steel than to make hard steel, and it may be, therefore, that the difficulties of manufacture will help to establish the limit of softness.

As to the second question: Is there any conception which helps us

to understand why it is that the softer rails give the better wear? I will say that in view of our absence of knowledge as to exactly what quality or combination of qualities of steel wear is a function of, the answer to this question becomes difficult. Nevertheless, we may possibly get a little help upon this point by devoting a few moments to the consideration of what takes place when steel suffers loss of metal by wear. What then is the condition of the surfaces involved in wear? What are the forces which act, and what are the strains produced?

If I understand the matter correctly, no two surfaces ever have been made, or can be made, that are absolutely smooth. At the very best, the smoothest surfaces are made up of elevations and depressions, very minute, it is true—perhaps almost infinitesimal and entirely incapable of measurement—but, nevertheless, real elevations and depressions. When these elevations and depressions are tangible we call the surface rough; when they are infinitesimal we call it smooth. If, now, this reasoning is correct, the surface of the head of a rail, as well as that of the circumferences of the wheels above it, are made up of elevations and depressions, which, when the two surfaces are in contact, must more or less fit into one another. And it is this fitting in of the minute elevations and depressions of the two surfaces that gives rise to friction. If the two surfaces were absolutely smooth, there would be no friction, and, consequently, no tractive power in the locomotives, nor would the wheels under the cars turn around. Friction of this kind we are accustomed to call rolling friction. In reality, then, a rail and the wheel which rolls above it may be regarded as a rack and pinion with infinitesimal teeth, lacking, of course, the element of regularity as to the position of the teeth which characterizes a rack and pinion. So much for the surfaces involved in wear.

Now, what are the forces involved in wear, and what strains do they give rise to? If I understand the matter rightly, there are two kinds of friction which may occasion loss of metal by wear in the case of rails: 1st. Rolling friction, which occurs when the wheels turn around and the trains move forward; and, 2d. Sliding friction, which occurs when the wheels turn around in the same place, as in the case of slipping drivers, or when the wheels do not turn around and yet move along the track, as in the case of sliding wheels. It is probable, however, that by far the largest portion of the loss of metal which

rails suffer is due to rolling friction; it will therefore, perhaps, be sufficient for our purpose to consider this case only.

The forces which act between the top of the head of the rail and the wheels in rolling friction may, it seems to me, be regarded as two in number. There is, first, a force acting directly downward, due to the weight of the locomotives and cars. This force may be regarded as a vertical force acting perpendicularly to head of the rail, and is in action both when the train is standing still and when it is in motion. Secondly, there is a force acting parallel to the head of the rail, due to the traction or impelling power of the locomotives. In the case of the driving-wheels, this force may be supposed to act in the direction opposite to that of the motion of the trains. In the case of all the other wheels, this force may be supposed to act in the direction of the movement of the trains, and it is antagonism to this force by the rail that causes all these wheels to turn around. In the case of the drivers, the amount of this force acting parallel to the head of the rail is sufficient to overcome the total train resistance; in other words, to cause the train to move. In the case of the other wheels of the train, acting individually, this force parallel to the head of the rail is small, being only that necessary to cause the wheels to turn around; in other words, to overcome journal friction. The force parallel to the head of the rail acts only when the train is in motion.

In the case of rolling friction, there is, of course, no wear except when both these forces are acting at once. But when two forces are acting at the same time their line of action must be regarded as in the direction of the resultant of the two forces. In the case we are considering we have a vertical force and a horizontal force acting at the same time, and the resultant line of action must, of course, be a diagonal. In other words, in rolling friction, if the conceptions given above are correct, the line of action of the force which produces wear must be regarded as a diagonal to the line formed by the head of the rail. In the case of the driving-wheels, the diagonal is inclined toward the front end of the train, and in the case of the other wheels it is inclined toward the rear of the train.

Returning for a moment to the conception previously mentioned, that the top of the head of the rail and the surface of the circumference of the wheel are a rack and pinion, with infinitesimal teeth, but without regularity in the teeth, let us see what kind of strain would be produced in these minute teeth by a force acting diagonally to the line

of the head of the rail. I hardly see how we can avoid the conclusion that this strain would be a bending strain. In the case when the infinitesimal teeth of the wheel and rail did not fit into interstices, but struck against each other, in which case the ends of the teeth only would be involved in the strain, then surely the strain would be a bending one; and again, in the case in which the teeth mutually engaged each other as a rack and pinion, so long as there was motion of the train, which is a necessary condition of rolling friction, it seems to me that the strain would still be that of bending.

If we are right in regard to the nature of the surfaces involved in wear and the strains produced, wear is simply the breaking or pulling off of the infinitesimal teeth by the strains to which they are subjected. And here we see why it is that the softer rails give the better wear, for the harder the steel the more brittle it is; and the more brittle the steel the more readily will these infinitesimal teeth be broken off by the strains applied. On the other hand, the softer the steel the more readily will these infinitesimal teeth bend and flatten down under the strain without breaking off. Or, to make the statement general, if the nature of the steel is such that under the strains applied, whatever they may be, the teeth readily break off, which is characteristic of hard steel, the more rapid will be the wear; if, on the other hand, the nature of the steel is such that the teeth readily suffer distortion without rupture, bend and flatten down without breaking off, which is characteristic of soft steel, the slower will be the wear. The above is not offered as a complete solution of the problem of wear, but as possibly helping us to understand why it is that the softer rails give the better wear as we actually find they do.

The data which have been obtained in the work done on this series of rails make it possible to get at some very interesting results as to the rate of wear of rails under different conditions of service. Thus, for example, what is the comparative rate of wear of rails on grade tangents and on level tangents, or on grade curves and on level curves, or on curves as compared with tangents, etc.? These comparisons may be made by taking the average loss of metal per million tons of all the rails which did their service under any one set of conditions, and comparing it with the average loss of metal of all the rails which did their service under any other set of conditions. This of course involves the supposition that the sixteen rails, for example, which did their service on grade tangents were equally as good, taken as a whole,

as the sixteen which did their service on level tangents. Whether this supposition is absolutely true or not, I think the differences between groups of rails, that is the differences between the groups that did their service under different conditions, can hardly be great enough to lead us into serious error. The results of these comparisons is given in the following three tables. In these tables the first column denotes the kind of service to which the rails have been subjected, the second column denotes the number of rails from which the average loss of metal per million tons in column third is derived, and the fourth column denotes the ratio of the loss of metal for the kind of service between which the comparison is made.

TABLE NO. I.

EFFECT OF GRADE ON WEAR.

Kind of service.	No. of rails.	Average loss of metal.	Ratio.
Level tangent,	16	·0384	1 : 1·82
Grade tangent,	16	·0701	
Level curve,	16	·0706	1 : 1·80
Grade curve,	16	·1277	
Levels,	32	·0545	1 : 1·81
Grades,	32	·0989	

The above table shows the effect of grade on the wear of rails. The average grade for the rails on tangents was 64·75 feet to the mile, and, as will be observed, the rails on the grades lost 1·82 times as much metal as the rails on levels. The average grade for the rails on curves was 53·13 feet to the mile, and, as will be observed, the rails on the grades lost 1·80 times as much metal as the rails on levels. The average grade for the whole 32 rails which were on grades was 58·94 feet to the mile, and, as will be observed, the ratio of the loss of metal on levels and grades is as 1 : 1·81. This table may be roughly summed up by saying that within the limits of our experiments, and with a grade of not quite 60 feet to the mile, the rails on the grade lose about 80 per cent. more metal per million tons than the rails on levels.

TABLE NO. II.

EFFECT OF CURVES ON WEAR.

Kind of service.	No. of rails.	Average loss of metal.	Ratio.
Grade tangents,	16	·0701	1 : 1·82
Grade curves,	16	·1277	
Level tangents,	16	·0384	1 : 1·84
Level curves,	16	·0706	
Tangents,	32	·0542	1 : 1·83
Curves,	32	·0992	

The above table shows the effect of curves on the wear of rails. The average degree of curvature of the rails on grades was $4\frac{7}{8}°$, and, as will be observed, the rails on the curves lost 82 per cent. more metal per million tons than the rails on tangents. The average degree of curvatures on levels was $2\frac{23}{32}°$, and, as will be observed, the rails on curves lost 84 per cent. more metal than the rails on tangents. The average degree of curvature for the whole 32 rails on curves was $3\frac{51}{64}°$, and, as will be observed, the rails on curves lost 83 per cent. more metal than the rails on tangents. This table may be roughly summed up by saying that under the conditions of our experiments, and on about 4° curves, the rails on the curves lose about 83 per cent. more metal per million tons than the rails on tangents.

TABLE NO. III.

WEAR ON HIGH AND LOW SIDES OF CURVES.

Kind of service	No. of rails.	Average loss of metal.	Ratio.
Grade curve, low side,	8	·0801	1 : 2·19
Grade curve, high side,	8	·1754	
Level curve, low side,	8	·0500	1 : 1·82
Level curve, high side,	8	·0911	
Low sides,	16	·0650	1 : 2·05
High sides,	16	·1332	

The above table shows the relative rates of wear on the high and low sides of curves. The average degree of curvature of the rails on the grades was $4\frac{7}{8}°$, and the average elevation of the rails on the high sides of the curves was $4\frac{7}{8}$ inches, and, as will be observed, the rails on the high sides of the curves lost 2·19 times as much metal per million tons as the rails on the low sides of the same curves. Again, the average degree of curvature of the rails on the levels was $2\frac{23}{32}°$, and the average elevation of the outer rail was $2\frac{23}{32}$ inches, and, as will be observed, the rails on the high sides of the curves lost 82 per cent. more metal than the rails on the low sides of the same curves. Finally, the average curvature of all the rails on curves was $3\frac{51}{64}°$, and the average elevation of the outer rail was $3\frac{51}{64}$ inches, and, as will be observed, the rails on the high sides of the curves lost 2·05 times as much metal as the rails on the low sides of the same curves. This table may be roughly summed up by saying, that under the conditions of our experiments, with an average curvature of about 4°, and an average elevation of the outer rail of about 4 inches, the rails on the high sides of the curves wear a little over twice as fast as the rails on the low sides of the same curves.

The data which have been obtained in the work done in this series of rails furnish likewise the means of getting at some interesting facts as to the life of rails. It is of course a difficult thing to say just exactly when a rail is so worn that it can fairly be called worn out. And yet we can perhaps make a few assumptions which, although not absolutely definite, may aid in throwing some light on this interesting question. If we take any standard 67 lb. rail, and suppose that the head is worn off on tangents or on the low sides of curves until it is from $\frac{1}{4}$ to $\frac{3}{8}$ of an inch lower than it was when rolled, the amount of metal worn off will vary from about 6 lbs. to 10 lbs. per yard. On the other hand, if the rail has done service on the high sides of curves, the amount of metal worn off when the side of the head is worn up to the fish-plate will not be far from 7 lbs. per yard. But as I understand it, the present practice on the road is, when a rail has become worn on the high side of a curve up to the fish-plate, to remove it to the other side of the curve, and get two or three years more wear out of it in that position. For the very general conclusions that we are after, therefore, it will perhaps be sufficiently accurate to say, that a rail may fairly be said to be worn out when it has lost 8 lbs. of metal per yard. This assumption of course does not pretend to actually represent the loss of metal by wear which may or ought to be obtained from every rail before it is worn out. In the case of the old standard section, it is probable that rails may fairly be said to be worn out when they have lost in the neighborhood of 8 lbs. per yard. In the case of the new standard section (section of 1875) a loss of from 10 to 14 lbs. per yard would leave the rail in as good condition as a loss of 8 lbs. per yard from the old standard section. In the following data, however, a rail is assumed to be worn out when it has lost 8 lbs. per yard, and from the figures given it will be easy to obtain the life of rails for any other assumed loss of metal. It will, of course, be borne in mind that the figures which follow are strictly applicable only to rails of the same quality and doing service under the same conditions as those from which these figures are derived. The conclusions must therefore be regarded as entirely general.

The following table shows what, with such average quality of steel as this report deals with, may be expected from rails in the various conditions of service. The first column gives the number of the line in the table, the second column the kind of service, the third column the number of rails from which the average loss of metal in column

four is obtained, the fifth column the number of million tons required in order that the rail may lose 8 lbs. of metal per yard, the sixth column the life, or number of years the rail would last in the north track, and the seventh column the life, or the number of years in south track. These last two columns are computed on the supposition that the yearly tonnage on the north track is 5,000,000 tons and on the south track 8,000,000 tons. On the north track, during the $9\frac{1}{2}$ years previous to July, 1879, the average annual tonnage, including of course the locomotives and the weight of the cars as well as their loads, has been 4,904,000 tons, while on the south track, during the 6 years previous to July, 1879, the average annual tonnage has been 7,985,000 tons.

TABLE IV.

THE LIFE OF RAILS UNDER DIFFERENT CONDITIONS OF SERVICE.

No. of line.	Kind of service.	No. of rails.	Average loss of metal.	No. of million tons to lose 8 lbs.	Life in years, North track.	Life in years, South track.
1	Level tangents, . .	16	·0384	208·4	41·7	26·1
2	Grade tangents, . .	16	·0701	114·1	22·8	14·2
3	Level curves, . . .	16	·0706	113·3	22·6	14·1
4	Grade curves, . .	16	·1277	62·7	12·5	7·8
5	Low side level curves,	8	·0500	160·0	32·0	20·0
6	High side level curves,	8	·0911	87·8	17·5	11·0
7	Low side grade curves,	8	·0801	98·9	19·8	12·3
8	High side grade curves,	8	·1754	45·7	9·1	5·7
9	Tangents, ...	32	·0542	147·6	29·5	18·5
10	Curves, ...	32	·0992	80·6	16·1	10·1
11	Levels, ...	32	·0545	146·8	29·4	18·3
12	Grades, ...	32	·0989	80·9	16·2	10·2
13	Low side curves, . .	16	·0650	123·1	24·6	15·4
14	High side curves, .	16	·1332	60·1	12·0	7·5
15	All conditions, . .	64	·0767	104·3	20·8	13·0
16	32 slower-wearing, .	32	·0506	158·1	31·6	19·8
17	32 faster-wearing, . .	32	·1028	77·8	15.6	9·7

In the above table the first eight lines show what may be expected from rails of the quality we are considering in all conditions of service, on the supposition that a rail is worn out when it has lost eight pounds of metal per yard, and that it carries five million tons per year in the north track and eight million tons per year in the south track. It is, of course, very simple to obtain from the figures given the life of rails under any other supposition. Thus, if the tonnage doubles, the life of the rails would of course be one-half as great

as that given above. On the other hand, if a rail is not regarded as worn out until it has lost 12 pounds of metal per yard, the life of the rail, the tonnage being the same, will be one-half greater than the figures given above. The remaining lines of this table, except the last two, show the life of rails under different combinations of the various conditions of service. They are given for the sake of information, and perhaps do not need any special comment or explanation. The first eight lines, of course, give the data that are most valuable. With regard to these data it will be noticed that on the high sides of grade curves, the average degree of curvature being $4\frac{7}{8}°$ and the average grade 64·75 feet to the mile, a rail of such average quality as those we are studying, and on the suppositions above given as to loss of metal and tonnage, may be expected to last a little less than six years in south track and a little over nine years in north track. On the low sides of the same curves the life is a little more than twice as long. This table may, perhaps, be best summed up by saying that rails have their shortest life on the high sides of grade curves, next on the high sides of level curves, next on the low sides of grade curves, next on grade tangents, next on low sides of level curves, and their longest life, as would be expected, on level tangents.

It is an interesting question whether it will be possible to get the high number of years' service given under some of the conditions above in actual practice. At first sight this seems improbable. But so far as loss of metal by wear is concerned, I see no escape from the conclusions given above, the conditions remaining the same. Other causes may, of course, take a rail out of service. And undoubtedly these other causes, such as crushing, breaking, distortion of head, and especially uneven wear, will all combine to make the average life considerably less than the above figures show. Nevertheless, the life of rails due to wear alone being known, it will, of course, be the aim to diminish the removal of rails from the track due to other causes as much as possible, and thus approximate closely to the best results.

One word more in regard to the life of rails. The last two lines in the above table give the same items of information, computed in the same way, as are contained in the remainder of the table. But in the first of these two lines the data are such as would be obtained if all the rails in the series had been as good as the average of the thirty-two slower-wearing or better rails, while the second of these two lines shows what would result if all the rails had been the same as the average

of the thirty-two faster-wearing. As will be observed, if all the rails in the series had lost metal or worn as fast as the average of the thirty-two faster-wearing rails, the best that we could expect under the suppositions given, as to loss of metal and yearly tonnage, would be a tonnage of about seventy-eight millions and a life of a little less than ten years in south track. On the other hand, if all the rails had been as good as the average of the thirty-two slower-wearing or better rails, the tonnage and life would be just about double these figures. In other words, the slower-wearing rails are on the average about twice as good in quality as the faster-wearing, and would consequently give about twice as high a tonnage and twice as long a life.

These data may be put in another form, which perhaps will be the fairer way of looking at them. The average life of the sixty-four rails we are studying, on the supposition that they are worn out when they have lost eight pounds per yard, and that the yearly tonnage in south track is eight million tons, is, as is seen in third line from bottom of above table, thirteen years. If, now, we were able to obtain steel rails as good in average quality as the thirty-two slower-wearing rails in our series, this average life would be almost twenty years, or an increase in life of over 50 per cent.

The question now arises, How shall the results of this work be utilized? I think it is entirely evident that the direction in which effort must tend in order that these results may be utilized is toward securing softer steel. And just here, perhaps, is the proper place to notice what seems to me a very important point. As you know so well this is our second investigation upon steel rails. The first investigation, as has already been stated, dealt principally with the question as to the relation between the chemical and physical properties of steel rails and their power to resist crushing and fracture in track. The conclusion reached, as you remember, was that the softer rails are less liable to crush or break in service than the harder ones. Now in this second investigation we find that the softer rails give the better wear. In other words, the softer steel makes rails which are not only less likely to crush or break in service, that is, rails that insure greater safety in the track, but also the softer steel makes rails that give better wear. And, unless the conclusions upon these two points can be overthrown by an equal amount of work, as carefully and conscientiously done, I do not see how the Pennsylvania railroad can, in the future, do otherwise than use every effort to secure softer steel in its rails.

And for this purpose I can see nothing better than that for the future the rails shall be bought on specifications, and subject to inspection and test before they are accepted. What specifications, therefore, does it seem wise to propose?

1st. As to chemical composition. It seems to me that it would be entirely philosophic to take as the specifications for the chemical composition the average analysis of the 32 slower-wearing rails in the series we are studying; especially as this analysis confines the work done on the 25 samples of steel rails in the first report to you on this subject. This average analysis is as follows:

Carbon, . . .	0·334 per cent.
Phosphorus, . .	0·077 "
Silicon, . . .	0·060 "
Manganese, . .	0·491 "

But, as was stated in that report, the amount of phosphorus above given is rather lower than the manufacturers of steel rails in this country can comfortably work. It would perhaps, therefore, be wise to increase the amount of phosphorus a little and diminish some of the other hardeners proportionally. In view of this reasoning, therefore, I see nothing better than to re-recommend the specifications as to chemical composition in our first report, as follows:

Phosphorus, not above, . .	0·10 per cent.
Silicon, not above, . .	0·04 "
Carbon, between 0·25 per cent. and 0·35 per cent., with an aim at, .	0·30 "
Manganese, between 0·30 per cent. and 0·40 per cent., with an aim at, .	0·35 "
Sulphur and copper, . .	No specifications.

2d. As to physical tests and inspection. What physical tests and inspection shall the Pennsylvania railroad prescribe for its rails? The inspection which is now practiced seems abundant to exclude rails whose defects are evident to the eye. As to the quality of the steel something further seems to be needed. Now, a physical test to determine the quality of the steel must, in the first place, be of such a nature that it will protect the consumer; that is, enable him to reject such steel as is not fitted for the purpose for which it is designed to use the metal. In the second place it must be practicable; that is, it must not require too elaborate appliances nor too much labor to make the test; and finally, it must not be so severe on the manufacturers as to

cause unnecessary hardship and loss in manufacturing the metal. What physical test now will fill these requirements? The data which have been obtained in the work done on the rails we have been studying enable us to prepare specifications which will make it possible to use any one of these four kinds of tests, viz., torsion test, tensile test, shearing test, or bending test. Thus we could, and if so desired will, prepare specifications leaving it optional with the steel-rail manufacturers which test shall be used. And I may be permitted to say here, that if the torsion test is chosen the data of the slower-wearing rails so strongly confirm the position taken in the first report to you on this subject that I do not see how anything else could be done than to re-recommend the specifications of the torsion test in that report.

But in looking over the physical tests of this series, I think it is plainly evident that the shearing tests and the bending tests bear a closer relation to the loss of metal per million tons than any of the other tests. It is true that there are some anomalous cases even in these tests; some cases that do not exactly fall into line. In the case of the bending tests this may in part, perhaps, be accounted for by the fact that these tests were made on samples cut from the web of the rail instead of from the head, where the wear took place. Mr. Cloud has found by a series of companion tests, made from samples cut from the head and web of the same rails, that the physical qualities of the metal in the head differ somewhat from that in the web, and, as has just been said, this may to a certain extent explain some of the anomalous cases in the series of bending tests. This closer agreement, however, between wear and the physical qualities of metals which are measured by shearing and bending, suggests that, perhaps, a shearing or bending test would be the best one to apply. And here I would like to express my judgement that the test suggested by M. J. T. Smith, General Manager of the Barrow Hematite Steel Works, several years ago, is, all things considered, the best physical test ever proposed for the examination of steel rails. Mr. Smith proposed that the fish-plate holes should be punched by a registering press, and that the quality of the steel should be judged by the pressure required to punch these holes. With the proper specifications as to the upper and lower limit of the punching strain, both steel that was too hard and steel that was worthless, from being spoiled in manufacture, could be rejected. And, still further, this punching test would be a shearing strain, and, at the same time, would enable a judgment to be formed

of the quality of the steel in every rail. A slight reaming of the holes after they were punched would remove the ring of metal injuriously affected by the punch, and leave the rails uninjured as to strength at the fish-plate holes. Unfortunately, however, with present appliances the punching test seems to be impracticable.

It would perhaps, therefore, be best to apply a bending test, especially if our analysis of the strain applied to the infinitesimal teeth of the rail in rolling friction is a correct one. It will be remembered that the theoretical considerations of what takes place in wear seems to plainly indicate that the strain applied to the minute elevations on the rail is a bending strain. If this is true, there could be no better test of the quality of steel to resist wear than a bending test.

Now there are four ways in which a bending test could be applied.

1st. The drop test. This test is in good use in England at the present time to determine the quality of steel for rails. But the disposition in this country, so far as my knowledge goes, seems to be to regard it as a somewhat crude test, and it is, therefore, being laid aside in favor of more accurate modes of testing.

2d. A piece of steel hammered out from a crop-end could be bent under a steam-hammer. This test was in use on the Pennsylvania railroad for some time to determine the quality of the steel, but its crudity, and the fact that the metal was manipulated after it had been rolled, and thus did not represent the steel in the rails, seem to me very justly to have led to its abandonment.

3d. A crop-end or piece of the rail just as it was rolled may be bent in a testing-machine. With proper specifications this test would undoubtedly give entirely satisfactory results. With a proper upper limit as to the maximum load required to bend the rail, steel that was too hard could be rejected, and, with proper specifications as to the amount of deflection the steel should stand before rupture, steel that was worthless from being spoiled in manufacture could likewise be rejected, and thus only good material be received. We would be glad to prepare specifications for this test if desired.

4th. A test-piece cut from the rail could be bent in the testing-machine. This test would undoubtedly likewise give us entirely satisfactory results. And for this test we have the data already at hand from which to prepare specifications. One or two things, however, should be borne in mind: 1st. The tendency of the steel rail manufacturers in this country at the present time is to make steel much

harder than is desirable, if our conclusions as to the wearing quality of steel are correct. This tendency must of course be recognized in preparing the specifications. And, in my judgment, this tendency will best be met by assigning an upper limit to the maximum load in the bending test. 2d. While our desire is to secure softer steel for rails, it must be remembered that steel may be so manipulated in manufacture as to render it very inferior in quality. Our specifications must therefore enable us to reject such worthless steel. This, in my judgment, will be successfully accomplished by requiring the test-pieces to bend to a certain angle before rupture.

I would, therefore, recommend that on all steel for rails received by the Pennsylvania Railroad Company in future the following bending test be prescribed: The test-pieces shall be cut from the web of the rail and shall be 12 inches long, $1\frac{1}{2}$ inch wide, and $\frac{1}{2}$ inch thick. These pieces shall be tested in the manner prescribed in the preceding part of this report, and on all steel accepted these test-pieces shall stand a maximum load of not over 3,000 pounds, and shall bend not less than 130° without rupture.

It gives me pleasure to make acknowledgments for services rendered to those who have assisted in this investigation. Mr. J. W. Cloud, engineer of tests, has had charge of the physical tests and measurements, and has either made them himself or had them made under his supervision. He has also from time to time made suggestions and contributed to the development of the ideas that have been worked out during the investigation in so many instances that it would be difficult to enumerate them. Mr. A. O. Dayton, assistant engineer of tests, made all the measurements with the planimeter, and likewise assisted in making the physical tests. Mr. H. L. Wells, at present chemist of the Colorado Iron and Coal Company, Pueblo, Colorado, did quite a portion of the chemical work. The phosphorus and manganese were principally determined by him. The silicon was determined by Mr. Wells and myself working together. The carbon was determined by myself. The services of my assistant, Mr. Theo. J. Lewis, have been almost invaluable in every part of the investigation. His patience, industry and careful attention to details can only be appreciated by those who know how laborious has been the work of collecting and working out into its present form the information herewith presented.

Very truly yours,

CHARLES B. DUDLEY, *Chemist.*

EXPERIMENTS ON THE STRENGTH AND STIFFNESS OF SMALL SPRUCE BEAMS.

By F. E. Kidder, B.C.E.

Read at the American Academy of Arts and Sciences, by Prof. Chas. R. Cross, S.B., February 9, 1881.

The following experiments were made in the physical laboratory of the Massachusetts Institute of Technology, being a part of the fourth year work in the course in architecture.

The object of the experiments was to determine the moduli of elasticity and of rupture in small beams of white spruce (*Abies alba*), and such other information as might be derived from the data obtained.

The machine used for the purpose consists of two solid wooden frames, carefully leveled and placed 40 inches apart. Upon the top of each frame is placed a movable plate of iron, which is carefully adjusted, so that the two plates shall be directly opposite each other, and exactly 40 inches apart between the faces. These plates form the supports for the beams.

The loads were applied by means of a scale-pan suspended from a $\frac{3}{4}$-inch bolt, which rested upon the centre of the beam. By means of an iron strap suspended from a horizontal beam placed above the test piece, and resting on two screws, the bolt from which the load was suspended, could be raised from or lowered upon the test piece as easily and gradually as could be desired.

The deflections of the beams were measured by means of a micrometer screw reading to $\frac{25}{10000}$ of a millimetre, or $\frac{1}{10000}$ of an inch. As the bolt from which the load was suspended rested on the centre of the beam, it was necessary to measure the deflections at a distance of one inch from the centre, but the deflections used in calculating the values of the modulus of elasticity were corrected, so as to give the deflection at the centre, supposing the curve assumed by the beam to be the arc of a circle, from which, in fact, it deviates but little under such small loads. In reading the micrometer, the principle of electrical contact was taken advantage of.

The chances for error in using the machine are as follows:

In measuring the deflections $\frac{1}{10000}$ of an inch. In the breaking load, possibly 1 pound; but in the small loads there could be no appreciable error. In measuring the dimensions of the test pieces $\frac{2}{1000}$ of an inch.

The experiments were conducted with the utmost care, and every possible precaution taken to prevent errors.

In arranging for the experiments, and while making them, the writer was greatly assisted by Mr. Holman, of the Massachusetts Institute of Technology, to whom he extends hearty thanks.

TABLE I.

No. of test piece.	Clear span. l.	Breadth. B.	Depth. D.	E	E_1	R	Centre breaking w't for beam 1''×1''. A.
	Ins.	Ins.	Ins.	Lbs.	Lbs.	Lbs.	Lbs.
1	40	1·475	1·45	1,731,000	1,657,000	11,380	632
2	40	1·445	1·52	1,556,000	1,528,000	10,330	574
3	40	1·469	1·448	1,765,000	1,732,000	10,710	595
4	40	1·42	1·498	1,736,000	1,636,000	10,830	601
5	40	1·45	1·485	1,688,000	1,578,000	11,980	665
6	40	1·48	1·44	1,795,000	1,686,000	11,040	613
7	40	1·464	1·46	1,682,000	1,561,000	10,570	587
8	40	1·42	1·48	1,647,000	1,556,000	11,280	626
9	40	1·46	1·46	1,704,000	1,638,000	11,180	621
10	40	1·441	1·46	1,616,000	1,550,000	12,440	691

Average value of E, 1,692,000 lbs.; of E_1, 1,612,000 lbs.
" " R, 12,170 lbs., of A, 620 lbs.
Weight of cubic foot, 25·7 lbs.

The pieces of wood experimented on were sawn from a spruce plank that had been cut in Eastern Maine in the spring of 1880, and the following summer shipped to Boston, where it had lain in the open air until it was cut up in October. The pieces were carefully planed to an approximate size of $1\frac{1}{2}$ inches square, and 4 feet long.

They were nearly all straight grained, and had but few defects, and

in testing the beams they were placed so that the defects should have the least possible effect upon the strength of the beams.

The exact dimensions of the test pieces are given in Table I.

In making the experiments, each beam was first subjected to a load of 30 pounds and the deflection noted. The weight was then left on the beam for a period of time varying from 1 to 4 hours, and in one case 44 hours, and the deflection again noted. The load was then removed from the beam, and the set noted, after which the beam was allowed a certain time to recover from the set.

After the piece had returned, or at least nearly returned, to its original position, it was subjected to a load of 40 pounds in the same manner.

Table II gives the deflections of each piece under the loads of 30 and 40 pounds, both immediately after the weight was applied, and after it had rested upon the beam the length of time designated.

The value of the modulus of elasticity, calculated from these deflections, is also given.

Modulii of elasticity, obtained from the deflection of the beams immediately after the weight was applied, have been denoted by E, and those obtained from the deflection of the pieces, after the weight had been applied one or more hours, by E_1.

Table I gives the values of E and E_1, for each piece, obtained by taking the average of the values given in Table II.

The values of E were computed by the formula $E = \frac{W l^3}{4 \Delta B D^3}$, in which W denotes the weight in pounds producing the deflection; l the clear span in inches; Δ the deflection of the beam at the centre; B the breadth of the beam, and D the depth, both in inches.

After all the beams had been tested in this way, piece No. 3 was again put in the machine and subjected to a load of 100 pounds, which was allowed to remain upon the beam for about two hours, the deflection being measured directly after the weight was applied, and just before it was removed. The beam was then allowed a certain time to recover its set. In two cases the beams, after having been subjected to a load of 100 pounds, finally returned to their original position, and it appeared probable that all would have done so had sufficient time been allowed for the purpose.

After the piece had nearly recovered from the effects of the load of

100 pounds, a load of 150 pounds was put on the beam, and gradually increased until the breaking point was reached.

The remaining pieces were tested with a load of 100 pounds in the same way, and then subjected to a load of 400 pounds for one or two minutes, for the purpose of getting the deflection under that load, and immediately after subjected to the full load of 500 pounds, which was gradually increased until the piece broke. As the load approached the breaking weight, it was increased by the addition of only one or two pounds at a time, so that the breaking weight could be obtained with sufficient accuracy. In fact, the breaking weight is so much modified by the time occupied in breaking the beam that it is difficult to ascertain exactly what it really is. For any load, over three-fourths of what is called the breaking weight, would probably break the beam if applied for a sufficient length of time.

Table I gives the values of the modulus of rupture of each piece, computed by the formula $R = \frac{3}{2}\frac{Wl}{BD^2}$, in which R denotes the modulus of rupture; W the breaking weight of the beam, and the other letters have the same significance as in the formula for E. The load which would break a beam of the same wood, one inch square, and one foot between supports, if applied at the centre, is also given in the same table. This load is $\frac{1}{18}$ of the modulus of rupture.

When the weight of 400 pounds was applied to piece No. 7, it immediately cracked at a gnarl in one of the lower edges, about three-fourths of an inch from the centre of the beam. As it was thought that the beam would soon break entirely, the load of 400 pounds was allowed to remain on the beam; but at the end of 100 hours the deflection had only increased 0·2224 inch, and, as it was evident that it would at that rate take a long time for the beam to break, the load was then gradually increased until the piece broke at 550 pounds, giving a modulus of rupture considerably above the average. It was noticed in this beam that the deflections under loads above 500 pounds were considerably greater than in the other beams under the same loads.

Piece No. 5 gave a very high breaking weight, and broke very suddenly, more like the harder kinds of wood. The fracture was very perfect; the upper half of the fibres being very evidently compressed, and the lower half suddenly pulled apart, with almost no splintering. This piece had a small knot on the upper side, 5 inches from the centre

of the beam, but it appeared to have no effect upon the strength of the beam.

TABLE II.

No. of test piece.	Weight. W.	Time applied.	Deflection. Δ.	E	Weight. W.	Time applied.	Deflection. Δ	E
	Lbs.	H. M.	Ins.	Lbs	Lbs.	H. M.	Ins.	Lbs.
1	30	0 00	0·0610	1,744,000	40	0 00	0·0826	1,719,000
1	30	2 25	·0630	1,689,000	40	3 30	·0873	1,627,000
2	30	0 00	·0616	1,536,000	40	0 00	·0801	1,576,000
2	30	1 45	·0639	1,481,000	40	3 00	·0842	1,499,000
3	30	0 00	·0606	1,774,000	40	0 00	·0815	1,757,000
3	30	1 00	·0610	1,759,000	40	1 00	·0840	1,705,000
4	30	0 00	·0582	1,725,000	40	0 00	·0765	1,748,000
4	30	2 25	·0616	1,627,000	40	3 20	·0813	1,645,000
5	30	0 00	·0580	1,740,000	40	0 00	·0774	1,637,000
5	30	2 30	·0618	1,630,000	40	44 15	·0874	1,527,000
6	30	0 00	·0605	1,792,000	40	0 00	·0862	1,676,000
6	30	3 00	·0639	1,697,000	40	5 12	·0803	1,799,000
7	30	0 00	·0624	1,683,000	40	0 00	·0833	1,682,000
7	30	3 00	·0663	1,584,000	40	16 00	·0911	1,538,000
8	30	0 00	·0632	1,645,000	40	0 00	·0839	1,650,000
8	30	4 15	·0666	1,561,000	40	16 30	·0894	1,552,000
9	30	0 00	·0619	1,701,000	40	0 00	·0823	1,707,000
9	30	1 30	·0643	1,638,000				
10	30	0 00	·0661	1,614,000	40	0 00	·0881	1,618,000
10	30	2 00	·0691	1,544,000	40	1 15	·0915	1,556,000

Piece No. 4 broke in a rather peculiar manner. While under a load of 575 pounds the lower fibres, for about a depth of one-tenth of an inch, snapped apart, and the beam gradually settled down until the next layer of fibres had apparently the same deflection as had the lower

ones at the time of breaking, when they also snapped, making a layer of about the same thickness. In this way the whole lower half of the beam seemed to divide itself into layers of about one-tenth of an inch thick, and to break separately, under about the same deflection, so that the beam was a long time in breaking.

Observing that under every load that had been applied the deflection kept increasing with the length of time the weight remained on the beam, piece No. 7 was subjected to a load of 275 lbs. for 98 hours, during which time the deflection increased 0·079 in. The weight was then taken off, and the beam allowed to recover for 24 hours, when it had a set of ·0446 in. The same weight was again applied, and it was found that the deflection, obtained by taking the difference between the readings just before and after the weight was applied, was less than it was the first time that the weight was applied, and the rate of increase of the deflection was about the same as before. The beam was thus subjected to a load of 275 lbs. for 300 hours in all, and it was then broken in the same manner as the others. It was expected that the effect of such a severe strain on the beam for so long a time would diminish the strength; but, on the contrary, it appeared to increase it, as the beam gave a higher modulus of rupture than any of the others, although it did not appear to be of as good quality as many of them. The ultimate deflection of this beam greatly exceeded any of the others.

Table III shows the deflection of each beam under loads of 30, 40, 100, 400, 500 and 550 lbs. immediately after the load was applied and at a distance of one inch from the centre. The *italic figures* under each deflection show what it would be if Hooke's law held true, taking the deflection under 30 lbs. as the starting point.

From these experiments I think we may draw the following conclusions:

That the modulus of elasticity depends not only upon the elasticity of the material, but also upon the length of time that the load is applied.

That when subjected to loads not exceeding one-sixth of the breaking weight, spruce beams do not take a permanent set.

That even under very small loads, if applied for any length of time, there will be a temporary set.

That knots and gnarls in beams loaded at the centre, when not

within one-eighth of the span of the centre of the beam, do not materially affect the elasticity under small loads.

TABLE III.

No. of test piece	Deflections in Inches, under						Ultimate deflection, in inches.	Breaking Weight.
	30 lbs.	40 lbs.	100 lbs.	400 lbs.	500 lbs.	550 lbs.		
	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.		Lbs.
1	0·0610	0·0826	0·2071	0·8303	1·0906	1·2791	1·5646	588
	·0610	*·0813*	*·2030*	*·8120*	*1·0150*	*1·1165*	....	.....
2	·0616	·0801	·2043	·8177	1·0677	1·2811	1·3941	575
	·0616	*·0820*	*·2050*	*·8200*	*1·0250*	*1·1275*	....	.....
3	·0606	·0815	·2023	·8976	1·2764	1·48*	1·48*	550
	·0606	*·0808*	*·2020*	*·8080*	*1·0100*	*1.1111*	....	.....
4	·0582	·0765	·1929	·7929	1·1146	1·3197	1·4658	575
	·0582	*·0776*	*·1940*	*·7760*	*·9700*	*1·0670*	....	.....
5	·0580	·0774	·2004	·7876	1·0170	1·1827	1·5788	637
	·0580	*·0773*	*·1933*	*·7732*	*·9665*	*1·0631*	....	.....
6	·0605	·0803	·2138	....	1·2520	1·4662	....	565
	·0605	*·0806*	*·2016*	....	*1·0080*	*1·1088*	....	.....
7	·0624	·0833	·2083	·8961	1·3595	....	....	550
	·0624	*·0832*	*.2080*	*.8320*	*1.0400*	....	....	.....
8	·0632	·0839	·2102	·8315	1·1111	1.3331	1·5709	585
	·0632	*·0842*	*·2106*	*·8424*	*1·0530*	*1·1583*	....	.....
9	·0619	·0823	·2083	....	1·0800	1·2830	1·4254	580
	·0619	*·0825*	*·2063*	....	*1·0315*	*1·1346*	....	.....
10	·0661	·0884	·2220	·9276	1.1772	1·3775	1·81*	637
	·0661	*·0881*	*·2203*	*·8812*	*1·1015*	*1·2116*	....	.....

That the deflection is very nearly proportional to the load, far beyond the customary limits of strain, and that the modulus is consequently very nearly constant for all moderate deflections.

* Approximately.

That a high modulus of elasticity does not always accompany high transverse strength. For, as shown by Table I, piece No. 10, which had the greatest transverse strength, gave next to the lowest value of E.

That in spruce beams the upper fibres commence to rupture by compression under about four-fifths of the breaking weight, and the neutral axis, at the time of rupture, is very near the centre of the beam, as shown by the fracture.

That beams which are subjected to severe strains, for a long time, bend more before breaking than those which are broken in a comparatively short time.

That the modulus of elasticity of small spruce beams, of a quality such as is used in the best buildings, may be taken at from 1,600,000 to 1,700,000 lbs., and the modulus of rupture at 11,000 lbs.

The only other experiments on American spruce with which the writer is familiar are those made by Mr. Hatfield, on small beams, 1·6 feet between supports, and some experiments by Mr. Thomas Laslett, of England, on pieces of Canada spruce, 2 inches square and 72 inches between supports.

Mr. Hatfield gives as the average value of the transverse strength of a unit beam 612 lbs.,* which would give 11,016 lbs. for the modulus of rupture.

From data given by Laslett † we obtain as the value of R, 9045 lbs.

The value generally given for the modulus of elasticity of spruce is 1,600,000 lbs.

Length of the Second's Pendulum. C. S. Peirce has re-examined the estimates of the length of the seconds' pendulum at Paris, by Borda, Biot and Kater. He finds that if allowance is made for the inertia of the air which is dragged by the friction of the pendulum, the estimated length should be increased about $\frac{1}{100}$ of 1 per cent. He therefore regards the true length as 993·934 millimetres (39·132 in.) Faye compliments Peirce's investigation very highly, and he congratulates the French Academy upon the great accuracy of the previous experiments, which were conducted largely under its auspices, when the facilities for minute measurements were not so great as they are now.—*Comptes Rendus.* C.

* Hatfield's "Transverse Strains," Table XLII.

† "Timber and Timber Trees, Native and Foreign," by Thomas Laslett, Inspector to the Admiralty. London, 1875.

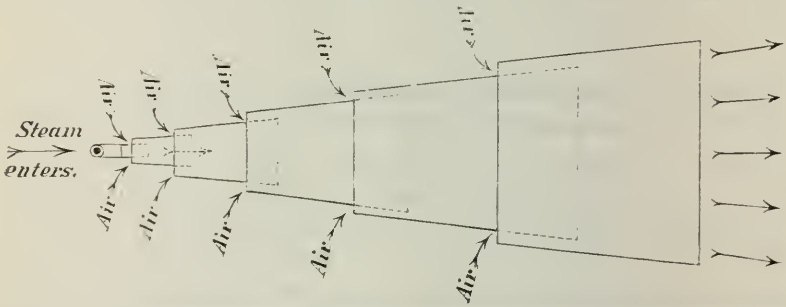
EXPERIMENTS MADE BY MR. SCHEURER-KESTNER WITH THE KŒRTING APPARATUS FOR THE INSUFFLATION OF AIR BY MEANS OF A STEAM-JET.

Translated* from the Bulletin of the Industrial Society of Mulhouse for the year 1878, page 674, by

Chief-Engineer ISHERWOOD, U. S. Navy.

The construction of Mr. Kœrting's apparatus is based on the principles employed in the Giffard injector and similar contrivances, with the single difference that in the latter a gas or vapor and a liquid are the reacting substances, while in the former they are gases or vapors. The Kœrting apparatus is used in various industries either for drawing in gases and then discharging them against pressure, or for mingling them with liquids; we have used it during two years in the factory of Thann for various purposes.

The necessity under which I found myself of knowing exactly the economic performance of this apparatus, that is to say, the effect produced and the expenditure of steam to produce it, caused me to make some experiments whose results seem worthy the attention of my colleagues of the Industrial Society.



The Kœrting apparatus (see sketch) is composed, abstractly, of a hollow right cone, from which frustra at intervals had been removed, and the remaining frustra, with heights proportional to their diameters, telescoped together in such manner that each is partly inserted into the next largest, an annular space being left between. This system is

*Some additions have been made to the author's description of the apparatus, and his calculations have been corrected for a known error. TRANSLATOR.

placed concentrically within an open-ended cylinder. High pressure steam by means of a small pipe is delivered at large into the smallest end of the smallest frustrum, traversing in succession the succeeding frustra, thereby drawing in through the annular spaces separating them the gas or air between them and their enveloping cylinder, the mixed steam and air being finally discharged from the base of the largest frustrum.

My experiments were made on a No. 2 Kœrting apparatus. The steam expended was ascertained by measuring directly the water vaporized in the boiler exclusively employed for furnishing it, and in which the water level and steam pressure were maintained strictly constant. The air drawn in was measured by an anemometer, the instrument I had used for several years to measure the air drawn in by coal burning and by sulphur furnaces. For this purpose a tube of 0·65618 foot diameter and 3·2809 feet length was placed at the receiving end of the cylindrical envelope into which one-half the length of the tube was inserted.

To verify, at least approximately, the performance of the apparatus I made some preliminary trials, in which I compared the relative velocity of the blades of the anemometer corresponding to observed variations of depression in the cylindrical envelope. The volume of air flowing through the latter, calculated from the number of revolutions made by the blades of the anemometer in a unit of time, agreed almost exactly with what could be deduced from the differences of depressions. The proper coefficient for the instrument being thus ascertained, I employed it in the following experiments.

The anemometer, made by Mr. Neumann of Paris, was numbered 225, and the formula for calculating the volume V of air is

$$V = 0·590562 \text{ foot} + 0·5675957 \text{ foot} \times n,$$

n indicating the number of revolutions made per second by the blades of the instrument.

The drawn in gas was discharged under a column of water 78¾ inches high into a wooden tank of 43⅓ inches diameter.

Steam pressure in the boiler, 57 pounds per square inch above the atmosphere.

Weight of water vaporized, 707·6834 pounds.

Duration of the experiment, 81 minutes.

Depression, 1·96855 inches of water.

Temperature of the gaseous mixture (air and steam), 185° to 186·8° Fahrenheit.

Temperature of the air, 77° Fahrenheit.

The temperature of the liquid did not exceed 186·6° Fahrenheit. The weight of steam condensed, determined from the increased volume of the water, was 224·8714 pounds; that is to say, about two-thirds of the steam were disengaged from the liquid, together with the overplus of air.

During the above time the axis of the blades of the anemometer made 95,561 revolutions, or 19·25123 per second.

Applying the foregoing formula, we find the velocity of the air in the adjutage to be $V = 0{\cdot}590562 + \overline{0{\cdot}5675957 \times 19{\cdot}25123} = 11{\cdot}517477$ feet per second. As the experiment lasted 81 minutes, the length of the gaseous cylinder drawn in was

(81 minutes × 60 seconds × 11·517477 feet =) 55974·9382 feet, the diameter of the adjutage being 0·65618 foot, the corresponding area to which is 0·3402212 square foot, the gaseous volume drawn in was 19043·8606 cubic feet.

Summing up the preceding, we have obtained against a resistance of 78¾ inches of water a gaseous volume of 19043·8606 cubic feet by an expenditure of 707·6834 pounds of steam, of which 224·8714 pounds were condensed.

As one pound weight of steam of the temperature of 186° Fahrenheit under the standard atmospheric pressure occupies 24·9877 cubic feet, we have for the centessimal composition of the volume of mixed air and steam discharged from the Kœrting apparatus

Air,	51·852
Steam,	48·148 *
	100·000

The greater the pressure of the boiler steam employed the less will

* The weight of a cubic foot of air at the temperature of 77° Fahrenheit, under the standard atmospheric pressure, being 0·0739359 pound, the weight of the air entrained by the 707·6834 pounds of steam was 1408·0250 pounds, from which we find that the gaseous mass of mingled air and steam discharged from the Kœrting apparatus consisted by weight centessimally of

Air,	66·55
Steam,	33.45
	100·00

TRANSLATOR.

be its expenditure in the Kœrting apparatus; in other words, the more the steam is expanded and its density decreased, the more will its useful effect be increased. This is what theory predicts and practice confirms. In another experiment, where the steam pressure was maintained in the boiler at 71 pounds per square inch above the atmosphere, instead of 57 pounds, the weight of steam expended for the same volume of air drawn in fell from 524·21 pounds to 445·33 pounds per hour, a gain of 17·713 per centum in favor of the higher pressure.

When the current of mixed air and steam is discharged at large into the open atmosphere, and has no pressure to overcome, the weight of steam necessary to produce a given air entrainment diminishes in a very high ratio. A gaseous mixture of air and steam can then be obtained whose temperature will not exceed that of the atmosphere more than from 18° to 22° Fahrenheit.

The following experiment was made with another Kœrting apparatus not working against pressure, the gaseous mixture being discharged at large into the open air.

Steam pressure in the boiler, 71 pounds per square inch above the atmosphere.

Weight of water vaporized, 1102·3106 pounds.

Duration of the experiment, 7 hours.

Temperature of the gaseous mixture (air and steam), 88° Fahrenheit.

Temperature of the atmosphere, 72° Fahrenheit.

The number of revolutions made by the axis of the blades of the anemometer was 12 per second.

$V = 0·590562 + 0·5675951 \times 12 = 7·4017104$ feet per second.

The velocity of the entrained air being 7·4017104 feet per second, and the duration of the experiment 420 minutes, the length of the gaseous cylinder drawn in is

(420 minutes $\times$ 60 seconds $\times$ 7·4017104 feet =) 186523·1021 feet.

The cross area of the adjutage being 1·3552253 square feet, the total volume of air drawn in is

$$186523·1221 \times 1·3552253 = 252780.8270 \text{ cubic feet.}$$

During the same time the weight of steam expended was 1102·3106 pounds, representing in volume (21·0059 cubic feet per pound) 23155·0262 cubic feet. The discharged gaseous mixture is then composed of 252780·8270 cubic feet of air and of 23155·0262 cubic feet of steam, or by volume centesimally:

Air, ...	. 91·6086
Steam, ...	8·3914*
	100·0000

These experiments show the extent to which the expenditure of steam can be reduced; and it is evident that by operating under other conditions, that is to say, with a greater expansion of the steam, the expenditure can be still more considerably reduced.

Novel Volcanic Phenomenon.—Within the space of ten months Mt. Etna had five abundant eruptions of smoke and sand, without any subsequent flow of lava. In one instance, after profound subterranean rumblings and numerous earthquake shocks, there appeared on the eastern side of the mountain a great cloud of vapors and ashes, which escaped by a crevice nearly three miles long. The snows melted suddenly around the summit of the mountain, jets of hot vapor escaped at many places, and the small muddy craters of the western declivity became very active, as is usually the case on the approach of a great eruption. But to the surprise of all observers, within thirty-six hours afterwards the volcano had returned to a state of perfect calm. Such a phenomenon has never before occurred within the memory of man. Vicenzo Tedeschi di Ercole attributes it to the existence of an immense opening, which appeared upon the mountain at the time of the eruption of May 26th, 1879. He concludes that a very strong pressure is required for the formation of lava, and that a great tension of gas is indispensable in order to raise the lava to the surface of a mountain. It appears probable, therefore, that there will be no reason to fear any further eruption in the cone of Etna as long as the present crevice is open.—*Ann. de Chim. et de Phys.* C.

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* The weight of a cubic foot of air at the temperature of 72° Fahrenheit, under the standard atmospheric pressure, being 0·0746120 pound. the weight of air entrained by the 1102·3106 pounds of steam was 18860·4831 pounds, from which we find that the gaseous mass of mingled air and steam discharged from the Kœrting apparatus consisted by weight centesimally of

Air, ...	. 94·48
Steam, . . .	5·52
	100·00

TRANSLATOR.

OBSERVATIONS ON THE WATER SUPPLY OF PHILADELPHIA.

By Reuben Haines.

It is well known that adverse criticism has of late years frequently been made on the character of the water supply of Philadelphia, both by residents and by visitors from other cities. To residents of Philadelphia it has not been satisfactory for a number of years for several reasons, such as shortness of supply during long summer droughts and consequent risk in case of fire; secondly, the frequent muddiness of the water so as to render it at times quite unfit for household use without domestic filtration, entailing some additional expense and trouble, which induces many to use well water in preference; and thirdly, well-grounded fears that the Schuylkill and Delaware river waters are, year by year, increasingly assuming a character which will eventually prove dangerously unwholesome, by reason of contamination with human sewage and factory refuse. It is admitted that a large amount of sewage passes into the Schuylkill above the several water-works, a fact capable of ocular demonstration; but it cannot be proved that any disease arises at present from that source, and the low death-rate of Philadelphia to a certain extent shows that its health is not materially affected by a contaminated water supply, especially when we consider the fact that many of the deaths are caused by blood-poisoning by sewer air through defective plumbing. Many people rely on the theory that most of the organic matter of sewage is destroyed during the flow of a few miles in the river, and since the Schuylkill water has never appeared to be unwholesome, they are naturally reluctant to approve of the expense of any change in the source of supply on this account.

But will this present wholesomeness continue? Is it true that the sewage of the Schuylkill is actually destroyed during the river-flow; and if so, to what extent does this destruction by oxidation and other means take place? Assuming that this oxidation may occur, may not a something causing infectious disease be liable to accompany this sewage, which will resist all destructive influences; and therefore, may not the mere fact of sewage contamination be a danger signal for

the future? These are questions of vital public interest; yet no effort has been made by Philadelphia for a practical solution of them, as regards the Schuylkill river, and no official examination has ever been made by this city of the experiments conducted in other places, concerning which various conflicting opinions have been expressed by different authorities.

A number of analyses of the Schuylkill water have been made by several chemists at different times, and the samples have been taken at various points. Enough has been given by Dr. Cresson in his report to the Chief Engineer of the Water Department, March 3d, 1875, to make it appear that the condition of the water is very variable, and that these variations do not correspond at all closely with the seasons. Sometimes the pollution is greatest during the summer months, while sometimes it is then at a minimum. The same may be said of the winter months. It is shown also, that the purity of the supply varies according to the location of the different pumping-stations.

From Dr. Cresson's analyses in the paper cited above, I have deduced the following averages, which I have calculated in parts by weight of ammonia in one million parts of water, the metrical form of statement being superior to all others for purposes of comparison:

	Free Ammonia.	Albuminoid Ammonia.
I. Average composition of water in Fairmount forebay from Feb., 1872, to Feb., 1875, by fourteen analyses in different months, . .	0·09	0·236
II. Average composition of the same from Feb., 1872, to Jan., 1873, by eight analyses in eight different months, ...	0·05	0·156
III. Average of same, from Jan., 1873, to Jan., 1875, six analyses, ...	0·12	0·447
IV. Average of same, from Jan., 1874, to Jan., 1875, four analyses, ...	0·12	0·541

From these results, considered by themselves, it might seem that the statement made by Dr. Cresson to the effect that the amount of sewage has been steadily increasing since 1872, until the water is occasionally charged with an amount of sewage exceeding that carried by the Thames at London is fully justified.

This is certainly an astonishing result considering that this water has been heretofore considered to be of standard good quality. A large

amount of sewage which passed into the river shortly previous to 1875 has since been diverted by a sewer on the west bank of the river. If there had been a sudden increase in this pollution it might partially explain the results of the analyses.

These results are, however, strangely in contrast to the analyses of the Fairmount water, made in 1875 by Booth and Garrett and published in their report for the Water Commission of that year, which gave in parts per million—free ammonia, 0·02; albuminoid ammonia, 0·03. This sample of water was, as they rightly remark, equal in purity to the water from the "artesian" well at Bryn Mawr, and was perhaps even somewhat better than the water from an "artesian" well in Germantown, 300 feet in depth, which I analyzed in August, 1877. It is to be regretted, however, that Messrs. Booth and Garrett give no information whatever as to the exact dates of their analyses, the condition of the weather previous to the collection of the sample, height of river, etc.; nor do they state whether their results are single analyses or averages of several samples; nor, again, the exact place where the sample was collected. These circumstances are so important for the proper comparison of analyses that much of the value of their results is thus destroyed.

It should also be said, that while Dr. Cresson does give some of these particulars, there are others he has not given.

I have made only a very few analyses of the Fairmount water, nevertheless the results may be of some interest. A sample was taken March 1, 1878, from a water-cock in a store at Seventh and Commerce streets, and therefore, represented the water as actually delivered to consumers. It gave in parts per million—free ammonia, 0·014; albuminoid ammonia, 0·056. This result agrees tolerably well with that obtained by Booth and Garrett. I never placed much reliance, however, on the results of a single analysis because *a priori* considerations would lead us to expect considerable variations at different times and seasons. By several analyses made during the present winter, of water collected at No. 315 Willing's Alley, I am lead to believe that the organic matter is often about four times as great as that indicated in my analysis just stated. It is possible, however, that these results may be invalidated if it is true that the water flowing through the city mains consisted of the mingled waters from several sources poured into the same reservoir before distribution.

Were the results given by Booth and Garrett correct as a yearly ave-

rage, their remarks upon the excellent quality of the Fairmount water in their report cited above would be fully justified. But while I have not the slightest doubt as to the correctness of any single analysis by these distinguished chemists, unfortunately I fail to find in their report any evidence that their analyses correctly represent the average quality of the river water, nor do they make any such statement, although their remarks would appear to be based upon this inference.

It may seem to some persons hardly worth while to criticise the report of the Water Commission after a period of five years has elapsed since its publication. But this objection may be disposed of by the consideration that almost nothing has been done toward adopting the recommendations of that report, or indeed, nothing at all to improve the purity of the Schuylkill water, except the construction of the sewer on the west bank of the river. Hence, the whole subject may be considered to be still in suspense.

Moreover, the City Councils have been repeatedly urged to carry into effect the suggestion made in that report to take water for the city supply from the Roxborough water works at Flat Rock dam, on the assumption that the water at this point was purer than the water at Fairmount, because it was taken from the river above the city limits.

Booth and Garrett's analyses, however, make it appear that in 1875. the Fairmount water was purer than that supplied by any of the other pumping works. If it had been stated that the samples were collected on the same day and under precisely similar circumstances, the evidence for this would have been much more conclusive.

My own analyses, although too few to form the basis of any definite conclusions, tend to confirm the impression drawn from those of Booth and Garrett. The amount of pollution was found to be less than the average condition during the whole year, 1872, by Dr. Cresson's analyses. My analysis of March 1st, 1878, given above, represents the state of purity frequently found as regards the water in the service pipes. An analysis of the Germantown supply from the Roxborough works, the sample being collected from the service pipe on the same day as the sample of Fairmount water above referred to, showed that the Germantown water contained twice the amount of organic impurity found in the water from Fairmount.

My analyses of the Germantown water have given the following

results, which are stated in parts per million, and all the samples taken from service pipes:

Date.	Place of Collection of Sample.	Free Ammonia.	Albuminoid Ammonia
June 18, 1877,	Corner of Main st. and Coulter st.,	0·127	0·091
" 22, "	" " "	0·034	0·124
" 23, "	" " "	0·032	0·116
July 13, "	" " "	0·034	0·140
" 13, "	Cor. Main st. and W. Walnut lane,	0·026	0·143
Feb. 4, 1878,	" " Coulter st.,	0·014	0·088
March 1, "	" " "	0·014	0·090
Nov. 21, 1879,	Wayne st. below Queen st.,	0·016	0·096
Dec. 7, 1880,	" " "	0·034	0·100
" 8, "	" " "	0·054	0·126
" 10, "	" " "	0·074	0·180
Jan. 11, 1881,	" " "	0·186	0·096
	Average of the above,	0·054	0·116

The following are a few analyses I have made of the city water, the samples also being drawn from the service pipes. Analyses stated in parts per million:

Mar. 1, 1878,	Seventh and Commerce streets,	0·014	0·056
Nov. 26, "	No. 315 Willing's alley,	0·030	0·120
" 27, "	" "	0·054	0·120
" 27, "	No. 1126 Ridge avenue,	0·066	0·200
" 27, "	Duplicate analysis of same sample,	0·074	0·206

It is thought it would be interesting to compare with these results analyses of the water of the Wissahickon and Wingohocken creeks:

Date.	Description	Free NH_3	Alb. NH_3
May 29, 1880,	Wissahickon creek, short distance above Norristown R.R. bridge,	0·240	0·290
July 14, 1877,	Wingohocken creek, East Branch, just above J. S. Haines' dam,	0·340	0·136
Aug. 1, 1877,	Wingohocken creek, West Branch, at East Walnut lane bridge,	0·112	0·192

The surface water was carefully excluded in taking these samples. The chlorine in each was respectively in the order given: —0·7, —0·85, and 1·15 parts in 100,000. The Wissahickon water was collected during a long continued drought and was quite clear and free from the mud usually found in it. The east branch of the Wingohocken received the drainage of a farm-yard above the place where the sample was taken. The west branch of this creek received the drainage

of pig styes and other surface pollutions. It was used for domestic purposes by a settlement of colored people.

We will now quote analyses by Booth and Garrett of the Schuylkill river waters given in their report of 1875, translating their figures into parts per million for the sake of comparison with the foregoing analyses.

	Free Ammonia.	Albuminoid Ammonia.
Fairmount, ...	0·020	0·030
Belmont, ...	0·100	0·087
Spring Garden, ...	0·299	0·149
Flat Rock (Roxborough works), .	0·125	0·087
Perkiomen creek, . . .	0·125	0·125

From Wanklyn's Water Analysis:

Thames and River Lea water, supplied by London companies, average of 44 samples, .	0·010	0·090
Thames water at Hampton court (London supply) before filtration, average of 2 samples, .	0·025	0·255
Thames river at London bridge at high tide, average of 3 samples, . . .	1·020	0·550

Dr. Cresson's analyses give the following as the average composition of the Schuylkill water at the Spring Garden, Belmont and Roxborough works:

Spring Garden, average of two analyses, .	0·61	0·835
Belmont, average of five analyses, .	0·45	0·454
Roxborough, average of two analyses, .	0·37	0·327

From this discussion the following conclusions may, I think, be legitimately drawn.

That the analyses by Booth and Garrett and Dr. Cresson of the water in the river at the works, and my own analyses of the water as delivered in the service pipes, all coincide in showing that the average quality of the water contained in Flat Rock pool is not likely to be any better than that of the water pumped at the Fairmount works; provided we exclude from this calculation Dr. Cresson's analyses for the year 1874, when, even if the results were not very approximately correct, there must have been some enormous and temporary pollution of the river. The figures in the analyses of July 24th of that year are, however, so very extraordinary that they excite the most serious

doubts as to their correctness, and they therefore cast suspicion on several of the other analyses also. For, to make this more evident, it may be said that the albuminoid ammonia in the analysis of the Spring Garden water of that date is equivalent to that in the average of twenty-seven samples of actual sewage taken at night from the sewers of Worcester, Mass., during prevalence of dry weather.*

	Free NH_3.	Alb. NH_3.
I. Average of 27 samples of sewage from three sewers in Worcester, Mass., taken at 6 A.M., 9 P.M. and 12 P.M. Night sewage, .	7·45	1·44
II. Average of 27 samples of the same taken at 9 A.M., 12 M. and 6 P.M. Day sewage, .	18·76	3·46
III. Dr. Cresson's analysis of the water at Spring Garden inlet, July 24, 1874, . .	0·29	1·45

These results are expressed in parts per million as heretofore in this paper

What is equally remarkable the water at Belmont inlet and Fairmount forebay were, according to Dr. Cresson's analyses, nearly as bad.

Bearing in mind that this albuminoid ammonia represents relatively the nitrogenous organic matter actually present both in solution and in suspension, Dr. Cresson's analyses appear altogether incredible. It will be impossible, we are sure, to make Philadelphians believe that at that time they were drinking liquid out of a veritable sewer. Moreover, his figures would represent the Fairmount water as twice, and the Spring Garden water nearly three times, as bad as the Thames river water at London bridge, which Wanklyn characterizes as "vile and stinking," having received a large part of the sewage of London.

For these reasons we must believe that some great mistake was made either in the mode of collecting the samples, concerning which the most scrupulous care is absolutely essential, or, in the mode of conducting the analysis, or in the calculation of the results.

In the comparison of the water from the different pumping stations, Dr. Cresson's analyses of November 7th, 1874, of the Fairmount and Roxborough waters collected on the same day, show a difference in favor of the former. Yet here again the amount of albuminoid ammonia in the Roxborough water is about five times as great as the amount found in the average of my own twelve analyses made in all

* *Vide* Fourth Report of Mass. State Board of Health, 1873, page 79. Compare also analyses of Boston Sewage. Idem p. 70.

seasons, from 1877 to 1881. It is about four times as great as Dr. Cresson found it to be only seven months afterwards. We must therefore consider the correctness of these particular analyses also as very doubtful, so far as representing the true condition of that part of the water supply of this city. Variations in quality from day to day and from month to month will undoubtedly occur according to the variations from time to time in the amount and character of the waste material of all sorts from the numerous factories, and also according to the amount of rainfall and consequent flushing of stagnant sewers, etc. But that these variations should be of the extent indicated by Dr. Cresson's analyses is incredible, especially in comparison with the results of the examination of a considerable number of the rivers of Massachusetts by authority of the Board of Health of that State, and where the pollution is of a somewhat similar character.

A report has lately been made on the character of the Ohio river water at Cincinnati, used for city supply; but it appears that, although the variations are exceedingly great the deductions to be drawn from them cannot properly apply to the Schuylkill, because the conditions affecting the flow and pollution of these two rivers are so very different. During September and October of the past year the water was, perhaps, what might be called tolerably good, which is stated to represent about the average condition for the greater part of the year; but during the latter half of November, and especially during December, it became frightfully bad—actually nearly as bad, according to the same series of analyses, as the water was at about the same time at the mouth of a sewer.* One of the ways in which the Ohio river is polluted is the frequent practice of the railroad men on the cattle trains of throwing the carcases of animals, that have died on the way, into the river at the bridges. Large numbers of these dead bodies are sometimes seen floating down the river. Analyses of water from wells several hundred feet deep on the river "bottoms," or flood ground, show this water to be as bad as the river itself, and that this earth contains enormous quantities of organic matter deposited during the frequent floods.

To return to the condition of the Schuylkill river my analyses show that a considerable amount of pollution exists at certain times in the supply from the Roxborough works. Yet the results of these analyses may possibly be affected in some degree by vegetable growth in the

* *Vide* Report of Analyses of Ohio river water by order of the Board of Health, Cincinnati, Dec. 20, 1880. By Prof C R Stuntz.

reservoirs and distributing mains, which is known sometimes to occur. On reference to these analyses it will appear that the largest amount of albuminoid ammonia for 1877 was 0·14 parts per million, and for 1880, 0·18, while the average for all was 0·11 parts per million. On referring to the analyses by Prof. Nichols of the water of Cochituate Lake, the purest water supply of Boston, published in the fifth report (1874) of the Massachusetts State Board of Health, page 117, we find that the maximum amount of albuminoid ammonia during the summer, autumn and early winter of 1873, was 0·13 parts per million, with an average of 0·11 parts. The samples in this case also were taken from service pipes at the Mass. Inst. Technology. While the average is the same in the two cases, the maximum is greatest in the Roxborough supply. If it be said that the difference is very insignificant it may be replied, that just this apparently insignificant difference in albuminoid ammonia will in the case of shallow wells constitute the difference between a well water which is passably good and one which may be dangerously polluted. In river waters, however, as has been explained in a preceding paper on this subject, we should not judge quite so strictly as in the case of shallow wells.

We should also consider in this connection the fact, which we believe to be true, that the Cochituate water contains extract of peat, since the gathering grounds for the supply of the lake are of a peaty character, and consequently a part of the albuminoid ammonia probably represents extract of peat which is by most observers considered quite innocent when in not very excessive amount. It will thus appear that the sewage contamination of the Roxborough supply is, in all probability, quite perceptibly greater than the Boston supply, inasmuch as no beds of peat are, I believe, to be found on the banks of the Schuylkill and its tributaries.

There is some reason to believe that either the Schuylkill river water was purer in 1875 than it has been during the period of my analyses, and perhaps, better than it was for the two years, 1873 and 1874, or else that the water at all the different pumping stations was at the time of Booth and Garrett's analyses perceptibly better than its average condition for 1875.

It should be stated that the maximum pollution indicated in my analyses was coincident in time with extremely low water in the river which had continued for some time, causing a probable concentration of the sewage, and therefore, not necessarily indicating an increase in

the absolute amount of sewage poured into the river. We cannot regard the water at these maximum periods as being of satisfactory character, since at such times a disagreeable taste, and even an odor, is perceptible and sufficiently marked to cause some people in Germantown to prefer well waters for drinking purposes.

It is proper to state that my own analyses have been conducted throughout with very great care as regards perfect cleanliness of apparatus, etc., and purity of reagents and with strict adherence to the directions given by Wanklyn in the third and fourth editions of his manual. The samples were collected according to the directions of Wanklyn and Frankland, the absolute cleanliness of sample bottles being especially regarded.

One point of some importance, to which, it is believed, no allusion has been made in published reports, is the probability that the quality of the water in Fairmount forebay usually may be said to be an average of that of the whole stream opposite to that pumping station, while this cannot be said of the water at any of the other works. This is owing to the fact that frequently during more than half the year no water flows over the comb of Fairmount dam, and consequently the whole stream, exclusive of a very small proportion necessary for the canal locks, passes through the forebay of these works to drive the turbines and supply the pumps, the whole flow of the river being utilized for these purposes, with the exception of what leakage occurs.

In the case of the other works the supply is taken from along shore, obviously the most impure part of the stream, and the water is pumped by steam power. It is evident that the Roxborough supply is also different from that at Fairmount, for although the former works are situated above Flat Rock dam, the water supply is here also pumped entirely by steam power and the river water flows quite continuously over the comb of that dam and the current is not therefore diverted to any great extent.

Hence, supposing that no polluting material were poured into the river between Flat Rock and Fairmount, and also supposing that the sewage was not oxidized to any extent during this flow we should naturally expect the water supplied from the Fairmount works to be of purer quality than that supplied from any of the other pumping stations.

This consideration will at once suggest a plan of obtaining a better

supply at all the upper works by extending the inlet pipe out into the middle of the river stream.

Setting aside the question of a more abundant supply for this city as not being within my province, and *considering solely its purity*, a very obvious method of improving its character is presented in the plan of constructing large covered sewers *on both sides* of the river, into which it shall be made compulsory to discharge every kind of artificial drainage and house-sewage and all factory refuse. These sewers to be effective at all should extend from near Flat Rock to a point below Fairmount, and were it not for engineering difficulties it would be better to extend them above Flat Rock dam. In this way the special pollution of Fairmount pool would be prevented and opportunity would be given for the oxidation of the sewage of Norristown, Conshohocken and other towns above Philadelphia. If it be true that this oxidation does take place and these intercepting sewers having been constructed, it would be possible to furnish the city with a purer supply than at present by drawing nearly the whole of the water from near Fairmount dam. By going up the river for the supply we would then only meet the sewage of Norristown, etc., which has been less thoroughly oxidized.

It is true that since the valley of the Schuylkill is destined in the far future to be densely populated, this plan will necessarily be only a temporary one. For we must look forward to a day when, if sewage irrigation is not soon established for all large towns, all our rivers will become no better than foul open sewers. It will also be only a question of time, of somewhat greater length, when the amount of sewage will counterbalance the effect of the greater volume of water in the Delaware river also.

Moreover, we have no reason to suppose at the present time that the specific cause of infectious disease may not co-exist with a small amount of organic pollution as well as with a large amount of it. We do not know whether this specific cause may not be capable of resisting all oxidizing influence for long periods of time, although the probabilities in some cases seem to be against this.

There is a marked tendency, of latter times, to give up rivers as a source of public water supply and to seek for it in natural and artificial lakes or large reservoirs in hilly districts. By this plan use is made of the storm waters of the region, and a water is obtained as nearly pure from the clouds by natural distillation as can be done on

a large scale. The hill country from which the supply is taken must be chiefly in woodland or pasture ground, and should not be in active cultivation. It is also to be noted that sewage contamination is less likely to occur in such a lake than in a river, for it has been observed in many places and among them, Massachusetts, that the centres of population extend usually along the river courses and seldom settle on the shores of small lakes. Hence, the plan of building an immense impounding reservoir on the drainage area, or, what is improperly termed the "water-shed" of Perkiomen creek, appears in some respects to be the best for the permanent future supply of Philadelphia, not only as regards its purity but also in regard to quantity of water. But the whole subject of this plan requires a far more thorough and extensive investigation than has yet been made. The teachings of sanitary science in all its bearings must be applied and investigations must be made as to what pollutions appear to exist at the present time in the Perkiomen creek and what is probable in the distant future. The question of the necessity of a State Board of Health must be considered, and the requisite control such a board should have in maintaining the purity of the water supply. For should polluting material enter this artificial lake, that which will not subside will be less likely to be thoroughly oxidized than in the river, and the pollution is, therefore, more important in the former case. Moreover, the continual subsidence of organic impurity in a reservoir which cannot be cleaned is an element of danger.

Comparative Expense of Lighthouse Service.—Emile Allard has published a comparison of the principal expenditures for lighthouse service in France, the United States and England. He finds the average annual cost of each light to be 3580 francs ($716) in France and 11,790 francs ($2358) in the United States. A large part of the economy in the French service is undoubtedly due to the difference in the cost of labor; but he also thinks that much of it is owing to the vigorous economy which the engineers of the department of bridges and highways bring to the execution of their labors, and to their careful avoidance of introducing luxurious arrangements, which do not contribute to manifest utility.—*Annales des. Ponts et Chaussées.* C.

A FOURTH STATE OF MATTER.

By Alexander E. Outerbridge, Jr.

A lecture delivered before the Franklin Institute, February 17th, 1881.*

While the past score of years has been fruitful in producing brilliant inventions like the telephone and other practical applications of science to utilitarian purposes, we should likewise recognize that great progress has been made in the less popular, though no less important, realms of purely abstract and theoretical science. It is a significant observation that all modern research tends to simplify nature's laws, to show their intimate correlation, and even to point still farther toward a complete unification or oneness of origin; thus the great forces of light, heat and electricity, though differing never so widely in their effect on our senses, have all been resolved into "modes of motion," and it would almost seem a natural inference from the general drift of modern scientific speculation that the now seemingly complex laws of nature's forces may all come, at some future day, to be included in the single study of the laws of motion. This little word *motion*, then, may be regarded as the key-note, the open sesame, of the modern philosopher. Even the so-called "inert matter" has not escaped its quickening influence. Speculations, at first regarded as visionary, are gradually formulating themselves into an at least plausible theory that "that which we call matter may be nothing more than the effect on our senses of the *movements* of molecules, or, as John Stuart Mill expresses it, a permanent possibility of sensation."

Sir Wm. Thomson, the profound scientist and mathematician, who may be regarded as a moulder of men's minds, has advanced a startling theory, which, however, is gaining constantly new proselytes, viz.:

* The Secretary of the Institute said, in introducing the lecturer, that a little more than one year ago Prof. Crookes, of the Royal Society, delivered a very remarkable lecture before the "British Association for the Advancement of Science," under the striking title "Radiant Matter." This lecture has excited universal scientific attention not only from the fact that whole classes of entirely new phenomena were developed, but also from the exceeding beauty and originality of the experimental illustrations. A few months ago a full set of Prof. Crookes' delicate apparatus was imported by Messrs. Queen & Co., of this city, and exhibited at one of the monthly meetings of the Franklin Institute, and a resolution was then passed inviting Mr. Outerbridge to lecture on the subject.

"That what we call matter may be only the rotating portions of something which fills the whole of space, *i. e.*, 'vortex motion of an everywhere present fluid.' "

Prof. Tait, following in the same path, says: "This property of rotation may be the basis of all that appeals to our senses as matter."

Prof. Crookes says: "From this point of view, then, matter is but a mode of motion." *

We are quite certain that many of the physical, as well even as some chemical changes which we observe in the sensible condition of matter, are due merely to movements among the molecules, or to motion of the atoms; thus the three commonly recognized states of matter, viz.: solid, liquid and gaseous, are consequences simply of the greater or lesser amplitude of motion of the constituent particles, and the question whether matter may exist in a fourth, or *ultra-gaseous* state, is the great scientific conundrum which Prof. Crookes has essayed to answer in the affirmative. He believes that he has discovered matter in a state as far removed from the gaseous as gas is from liquid or liquid from solid, and says: "In studying this fourth state of matter we seem at length to have within our grasp, and obedient to our control, the little indivisible particles of matter which, with good warrant, are supposed to constitute the physical basis of the universe." Whether Prof. Crookes' conclusions prove conclusive or not, we are indebted to his genius for a most brilliant investigation, teeming with possibilities

* "Motion, wherever we can directly trace its genesis, we find to pre-exist as some other mode of force. Our own voluntary acts have always certain sensations of muscular tension as their antecedents. When, as in letting fall a relaxed limb, we are conscious of a bodily movement requiring no effort, the explanation is that the effort was exerted in raising the limb to the position whence it fell. In this case, as in the case of an inanimate body descending to the earth, the force accumulated by the downward motion is just equal to the force previously expended in the act of elevation.

"Conversely, motion that is arrested produces, under different circumstances, heat, electricity, magnetism, light. From the warming of the hands by rubbing them together up to the ignition of a railway brake by intense friction—from the lighting of detonating powder by percussion, up to the setting on fire a block of wood by a few blows from a steam hammer; we have abundant instances in which heat arises as motion ceases. * * * The production of electricity by motion is illustrated equally in the boy's experiment with rubbed sealing wax, in the common electrical machine and in the apparatus for exciting electricity by the escape of steam. * * * * And similarly, motion may create light, either directly, as in the minute incandescent fragments struck off by violent collisions, or indirectly, as through the electric spark. 'Lastly, motion may be again reproduced by the forces which have emanated from motion.' "—*Synthetic Philosophy*, Herbert Spencer, p. 197.

for further development and giving us fascinating little vistas into unexplored regions of knowledge in that "border land where Matter and Force seem to merge into one another, the shadowy realm between Known and Unknown."

Inter-Molecular Spaces and Motion.

The porosity of all substances, even those which we commonly regard as most dense, proves that the constituent particles are not in contact. You remember the old experiment of the sphere of gold filled with water, on compressing the sphere drops of water oozed through the pores of the gold, *i. e.*, the spaces between the molecules.

A bar of iron, or other metal, may be readily compressed under a steam hammer, and we know that we do not diminish the size of the metallic particles one iota; we simply contract their house room, as it were, or crowd them nearer together.

The porosity of liquids may be readily proved,* while this property is one of the most striking characteristics of all gases.†

The idea that the constituent molecules of all bodies, whether in the solid liquid or gaseous state, are not at rest, but constantly moving, with enormous velocities, within certain boundaries, is so inseparably associated with our modern conception of matter in these three states that we may say, broadly, we believe firmly that no solitary molecule, whether constituting an infinitesimal portion of this great globe or inhabiting the most distant star, has yet found a lodging place where it may enjoy absolute rest. The particle was endowed at its creation with an irresistible antagonism to inertness. Constant motion is a very necessity of its being; the whirling and the clashing of the atoms goes on forever, like the music of the spheres, and this eternal dance of molecules can cease only with the utter annihilation of familiar matter.‡

* *E g.* A glass filled with water, to which may be added salt, sugar, etc.

† *E g.* Diffusion exhibited by Prof. Graham's experiment, etc.

‡ "The space covered by the motion of molecules has no more right to be called matter than the air, traversed by a rifle bullet, can be called lead.. * * * At the absolute zero of temperature the inter-molecular movement would stop, and, although *something* retaining the properties of inertia would remain, *matter*, as we know it, would cease to exist."—Letter to secretary of the Royal Society, W. Crookes, F.R.S. *Nature*, vol. 22, No. 7, p. 153.

"There is no repose of any kind in nature; its whole existence is a constant cycle in which every motion, the consequence of a preceding motion, becomes immediately

The change of matter from the solid to the liquid and then to the gaseous state is a most familiar occurrence. Let us try to form a clear mental picture of the movements, which the molecules undergo, whereby a body which is opaque like lead and rigid like iron becomes first plastic, then fluid, and then dissipates before our eyes as invisible vapor. Whence come these marvelous transmutations? A crude illustration may perhaps answer our question clearly and without scientific technicality. Let us imagine a hall crowded to repletion with dancers. It is evident that the movements of each person will be very circumscribed, he cannot advance far in any direction without jostling his neighbor; the result will be that, at the end of a given time, the individuals will all occupy the same relative positions in the room that they held at the beginning. The characteristics of the mass, therefore, have remained unchanged. This simile illustrates the motion of the molecules composing a solid body; they may be regarded as gyrating within small limits of space and constantly jostling each other; their paths of free movement being exceedingly small, the molecules cannot change places, and the characteristics of hardness, rigidity, etc., remain fixed.

Now, let us open the folding doors of the hall so that some of the dancers may overflow into the adjoining room; more freedom of movement is immediately afforded, the individuals change places constantly in this giddy whirl, and an observer in the gallery notices kaleidoscopic changes every moment in the characteristics of the mass. This represents the change of a solid body to the condition of a fluid.

If we continue to enlarge the boundaries, still greater changes will be observed in the mass; the unoccupied space becomes excessive, so that

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the cause of an equivalent succeeding one; so that there is nowhere a gap, nowhere either loss or gain."—Buchner.

"Matter is not like a carriage, to which the forces, like horses, can be put or again removed from. A particle of iron is, and remains, the same whether it crosses the horizon in the meteoric stone, rushes along in the wheel of the steam engine, or circulates in the blood through the temples of the poet. These qualities are eternal, inalienable and untransferable."—Du Bois-Reymond.

"In whatever way we may think of an original substance, there must always exist in it a system of mutual repulsion and attraction between its minutest parts, without which they would dissolve and tracelessly disappear in universal space."—Buchner.

"A thing without properties is a nonentity, neither rationally cogitable nor empirically exsisting in nature.—Drossbach.

"A force not united to matter, but floating freely above it, is an idle conception.—Moleschott.

"the mean free path" of each individual is greatly increased, and collisions can only occur because the waltz merges into a wild galop. This depicts the molecules of matter in the gaseous or third state.

If we should be able still further to isolate the individual dancers, or what is equivalent, to greatly decrease their number, each one would have so vast an allotment of space, or the "mean free path" of each would become so great that we could no longer observe the motions of a group *en masse*, but should have to confine our attention to one, or at least a very small number who could only communicate with or jostle against each other by an exercise of agility hitherto quite unsuspected, fairly entitling them to be called by a new name, as acrobats rather than dancers. Such may serve as a homely illustration of the modern idea of matter as it is said to exist in a "fourth" or "radiant" or "ultra-gaseous" state.

Divisibility of Matter.

In studying the character of the ultimate particles we are at once impressed with the extraordinary degree to which matter may be subdivided. Numerous illustrations might be given had we sufficient time and space to devote to this interesting by-path. "Some of these experiments (notably those of Faraday) present the curious anomaly of revealing to the physical sense of sight particles of matter, which are almost too infinitesimal for the mind's eye to conceive, thus seeming to reverse the order of scientific investigation, which usually prolongs the mental vision far beyond the region of possible physical revelation."*

By mere mechanical means the metal gold may be spread out into a leaf one three-hundred-and-fifty-thousandths of an inch in thickness.

By means of the battery films of gold have been deposited on copper and afterward detached, by dissolving the copper and floating the gold on glass slides, not exceeding one ten-millionth of an inch in thickness.

By the aid of the spectroscope particles of matter may be revealed which are invisible to the eye by the aid of the most powerful microscope.

We are assured that there are infusorial animals so minute that millions do not equal in bulk a single grain of sand.

Tripoli consists of the bodies of animalcules so small that a piece

* Divisibility of gold and other metals, by A. E. O., Jr., *Pop. Science Monthly*, May, 1877.

the size of a marble contains at least 1,000,000,000, or more than all the human beings on the globe. "Each animalcule possessed all the organs necessary to life, performing the functions of respiration, digestion and locomotion; how inconceivably small are the vessels through which the fluids of their bodies circulate, and what must be the size of the ultimate particles to nourish an animal whose totality is too small to estimate?"

Compared, however, to the size of the particles constituting cometary matter, those which we have here indicated are as coarse as the famous Philadelphia cobble stones contrasted with grains of sand upon the seashore.*

We might naturally suppose from these considerations that no absolute knowledge could ever be obtained regarding the nature of the infinitesimal particles of matter out of which worlds are formed; indeed, Brande has said "Of the ultimate nature of matter the human faculties can not take cognizance, nor can data be furnished by observation or experiment on which to found an investigation of it." Sir Wm. Thomson, however, has said more recently that they are "pieces of matter of measurable dimensions, with shape, motion and laws of action; intelligible subjects of scientific investigation."

The same eminent authority, undaunted by the intricate complexities of the problem, has after most careful research, and by the corro-

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* "From their perviousness to stellar light and other considerations Sir Jno. Herschell drew some startling conclusions regarding the density and weight of comets. These extraordinary and mysterious bodies sometimes throw out tails 100,000,000 miles in length and 50,000 miles in diameter. Now, suppose the whole of this stuff (*i. e.*, the matter forming the tail) to be swept together and suitably compressed, what do you suppose its volume would be? Sir Jno. Herschell would probably tell you that the whole mass might be carted away, at a single effort, by one of your dray-horses. In fact, I do not know that he would require more than a small fraction of a horse power to remove the cometary dust.

"After this you will hardly regard as montrous a notion I have sometimes entertained, concerning the quantity of matter in our sky. Suppose a shell to surround the earth at a distance which would place it beyond the grosser matter that hangs in the lower regions of the air, say at the height of the Matterhorn or Mont Blanc. Outside this shell we have the deep, blue firmament. Let the atmospheric space beyond the shell be swept clean and the sky-matter properly gathered up. What would be its probable amount? I have sometimes thought that a lady's portmanteau would contain it all. I have thought that even a gentleman's portmanteau, possibly his snuff box, might take it in. Whether the actual sky be capable of this amount of condensation or not, I entertain no doubt that a sky quite as vast as ours, and as good in appearance, can be formed from a quantity of matter which might be held in the hollow of the hand."—*Fragments of Science*, Tyndall, p. 444.

borative aid of at least four separate and distinct methods of calculation, all agreeing within certain limits of error, deduced approximately the size of the ultimate particle, which he tells us is not far from one five-hundred-millionth of an inch in diameter. To form an idea of the size of these molecules Sir Wm. Thomson has given this illustration: "Imagine a drop of rain, or a glass sphere the size of a pea, magnified to the size of the earth, the molecules in it being increased in the same proportion, the structure of the mass would then be coarser than that of a heap of fine shot, but probably not so coarse as that of a heap of cricket balls.

This estimate is commonly received by scientists as representing the average "coarse-grainedness" of matter.

Nature of Matter.

We are now prepared to ask what is the character of the ultimate particle? This is a problem which has exercised the philosophic mind for many generations. The celebrated Italian poet and philosopher, Lucretius, speculated on this topic, and left a work called "De Rerum Natura"—*of the nature of things*—embodying his views, which are interesting even at this day.

Sir Isaac Newton invented, or adopted, the theory that all matter was composed of little, hard, incompressible spheres. This theory afforded him a plausible and ingenious explanation of certain discrepancies which he found to exist between the real velocity of sound in air, as proved by experiment, compared with the theoretical velocity, as calculated by him, based upon seemingly correct data. The theory, however, was found to be untenable, and was long since overthrown.

The idea of motion, as associated intimately with matter in some vague and unexplained way, seemed to be intuitively felt to be a necessity even before the time of Faraday, and a reaction from the hard atom theory of Newton brought out a class of philosophers who maintained that the so-called atom was not material at all, but that there existed in space certain foci, or centres of force; these points were supposed to possess the properties of attraction, repulsion, etc., and to behave in other respects as the most approved atom. This idea is believed to have been at least countenanced, if not actually adopted, by Faraday.

The wonderful investigations of Helmholtz on rotary fluid motion paved the way for Sir Wm. Thomson's "Vortex Atom Theory."

Vortex Atom Theory.

This theory seems to explain many obscure phenomena, and, while we may hardly say that it is to-day the fully accepted creed of the scientific world, it is founded—not upon a rock—but upon the potent influence of the talismanic word motion, to whose eddying current the causes of so many of nature's grandest phenomena are now relegated.

"Sir Wm. Thomson's supposition is that the universe is filled with something which we have no right to call ordinary matter, but which we may call a perfect fluid, then, if any portions of it have vortex motion, they cannot part with it; it will remain with them forever, or at least until the creative act which produced it shall take it away again."*

This theory is far too complicated to admit of a clear exposition in a few words, but we will try to form an idea of it by means of a simple illustration. You know that a smoker may, by a peculiar adjustment of the mouth, accompanied by sudden muscular movements of the cheeks, skilfully exhale portions of the tobacco smoke in the form of beautiful opaque rings, which continue to rotate about their axes, gradually disappearing as the motion ceases; this is a vortex movement of the simplest order, and while it lasts the portions of the smoke forming the rings are separated and distinct from all the rest of the smoke in the room, and should this motion never cease (by the retarding influence of friction and gravitation) this smoke would remain differentiated forever and imbued with new properties in virtue of that vortex motion.

This crude illustration conveys the simplest idea of a most abstruse scientific speculation when referred to the hypothetical, all-pervading, perfect fluid, so essential to the existence of Sir Wm. Thomson's vortex atom.†

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* *Recent Advances in Science*, P. G. Tait.

† The subject of rotary fluid motion is such a forbidding one, from a purely mathematical point of view, that no one had done more than take a look at it, as it were, until Helmholtz gave us the fundamental propositions; splendid as they are, they are only a first step. Indeed, to investigate what takes place when one circular vortex atom impinges upon another, and the whole motion is not symmetrical about an axis, is a task which may employ perhaps the lifetimes for the next two or three generations of the best mathematicians in Europe, unless in the meantime some mathematical method, immensely more powerful than anything we at present have, should be devised for the special purpose of solving this problem.—P. G. Tait, *Recent Advances*, p. 298.

The numerous criticisms which have appeared of Prof. Crookes' remarkable lecture on radiant matter have all, strange to say, failed to recognize the fact, so clearly and completely stated by Prof. Crookes himself, that the original idea of an ultra-gaseous condition of matter is by no means a novelty, having been conceived by Faraday, who mentioned this extremely attenuated form as radiant matter as far back as 1816.*

The concluding portion of the lecture was devoted to an exhibition and explanation of Prof. Crookes' apparatus, contributed for this occasion by Messrs. Queen & Co. This consists, for the most part, of a number of small glass tubes and bulbs, from which the air has been exhausted to about one-millionth part of an atmosphere; fine wires are hermetically sealed into the tubes and a powerful induction coil supplies rapid electrical pulsations, under the influence of which the residual matter, or particles of air, and even the sides of the glass flash out into brilliant corruscations of phosphorescent light, of exquisite tints, exhibiting also mechanical and other effects of a surprising and entirely novel character.

Before showing the apparatus of Prof. Crookes, some very beautiful experiments with low vacuum tubes were exhibited by way of contrast with those which followed. The Crookes tubes included one showing the dark space where a plate of aluminium in the centre of the tube was the negative pole, and the radiant matter driven forth by the current only exhibited itself by light when in the part of the tube where the moving atoms were brought into collision with the sides of the tube or with each other. Two or three minute engines, showing the

*"It was more than sixty years afterwards that Prof. Crookes took up the subject in a way to make the phrase "radiant matter" familiar to the reading public, as well as to the guild of scientific men. This "fourth state," or extra-gaseous state, of matter was not produced in his experiments by the application of high heat, as in melting metals or turning water into vapor, but by pumping the air from glass tubes and vessels until only the one-millionth of an ordinary atmosphere was left within. In this condition what remained of the air in the tubes and vessels was so greatly diffused that each individual molecule of air had a million times more freedom of movement than in the natural condition of the atmosphere, and it is the very remarkable character of these enclosed molecules in a high vacuum when excited by external agencies, such as electricity, that has caused Prof. Crookes to designate them as "radiant matter," following Faraday, and "a fourth state of matter," following the phraseology of other scientists. The widely separated atoms are made to act with marvelous energy and to produce wonderful effects—they not only produce light and heat by their motion, but while invisible themselves, they are made to set visible things in motion and to produce other visible effects.—Editorial in *Public Ledger Supplement*, Feb. 19, 1881.

mechanical force of radiant matter, were also exhibited. One of them was a propeller wheel, which, at first, refused to revolve, though evidently agitated by the current. It was afterwards ascertained that the wire connection had been broken, and the accident afforded a good illustration of the theory of the action produced, the spasmodic movements of the wheel being due to the jumping of the current between the ends of the broken wire. The connections having been properly made, the wheel revolved. Another tube, which was projected from a lantern, has placed within it a miniature railway track of glass, on which there is a small wheel, like the side wheel of a steamboat, the blades or paddles being made of thin mica, and when the terminal wires at the ends of the tube are connected with an induction coil the infinitesimal molecules are shot from one end of the tube to the other, striking the blades on their way with such force, and exhibiting such rapid motion, as to carry the wheel from end to end and back in swift alternation as the current of electricity is reversed in polarity. The shadow tube was also shown. In this a Maltese cross is arranged in the tube, which intercepts the moving atoms and so produces a shadow on the end of the tube, which is illuminated outside of the lines of the cross by the impinging of the particles on the end of the tube. The cross being thrown down, the former shadow becomes brighter than the surrounding parts. It is supposed that the glass, having been subjected to the action, becomes less sensitive, so that the newly exposed parts for a time respond with greater vibrations and so give more light. Tubes showing that in the lower vacuum the particles travelled in direct lines between the terminals while in higher vacuums they were driven in straight lines directly from the negative pole, were also shown. Another tube was made with two chambers, in one of which were the electrodes and in the other a compound of potassium, capable of giving out a watery vapor. The tube was exhausted to the last degree, so that the passage of the current was not possible. Then the substance in the "annex" chamber was heated to make it give off atoms or vapor when the particles of the glass were set in violent motion and gave forth the characteristic green light. As the number of molecules and consequent collisions increased this color changed to the rosy hue seen in low vacuum tubes. As the vapors were recondensed the green light returned and finally the current ceased to pass and all was darkness. The last experiment was with a tube in which the radiant matter, passing from a cup-shaped negative terminal, was focussed, so

to speak, on a metal disc of iridio-platinum, heating it to redness. It was shown with this tube that the direction of the atoms could be deflected by a magnet. The experiments were as beautiful as they were instructive.

ACTION OF AN INTERMITTENT BEAM OF RADIANT HEAT UPON GASEOUS MATTER.

By John Tyndall, F.R.S.

Read before the Royal Society January 3, 1881.

The Royal Society has already done me the honor of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Professor Magnus and the late Professor Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question, the latest of whom,* while justly recognizing the extreme difficulty of the subject, and while verifying, so far as their experiments reached, what I had published regarding dry gases, find me to have fallen into what they consider grave errors in my treatment of vapors.

None of these investigators appear to me to have realized the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognize the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed, some time ago, the threads of the inquiry. The results shall, in due time, be communicated to the Royal Society; but, meanwhile, I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

After working for some time with the thermopile and galvanometer, it occurred to me several weeks ago that the results thus obtained

*MM. Lecher and Pernter, *Philosophical Magazine*, January, 1881; *Sitzb. der K. Akad. der Wissensch. in Wien*, July, 1880.

might be checked by a more direct and simple form of experiment. Placing the gases and vapors in diathermanous bulbs, and exposing the bulbs to the action of radiant heat, the heat absorbed by different gases and vapors ought, I considered, to be rendered evident by ordinary expansion. I devised an apparatus with a view of testing this idea; but, at this point, and before my proposed gas-thermometer was constructed, I became acquainted with the ingenious and original experiments of Mr. A. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies.

From the first, I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged upon by the beam. But if this be the case, and if gases and vapors really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds, while highly athermanous bodies would produce loud sounds; the strength of the sound being, in a sense, a measure of the absorption. The first experiment made, with a view of testing this idea, was executed in the presence of Mr. A. Graham Bell;* and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to embrace most of the gases and vapors employed in my former researches. My first source of rays was a Siemens' lamp connected with a dynamo machine, worked by a gas engine. A glass lens was used to concentrate the rays, and afterwards two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about 7 inches distant from the lens. A circle of sheet zinc, provided first with radial slits and afterwards with teeth and interspaces cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits pro-

* On the 29th of November; see *Journal of the Society of Telegraphic Engineers,* Dec. 8th, 1880.

duced the desired intermittence,* while a flask containing the gas or vapor to be examined received the shocks of the beam immediately behind the rotating disc. From the flask a tube of india rubber, ending in a tapering one of ivory or boxwood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. A. Graham Bell, the arrangement here described is rude; it is, however, very effective.

With this arrangement the number of sounding gases and vapors was rapidly increased; but I was soon made aware that the glass lenses withdrew from the beam its most effectual rays. The silvered mirrors employed in my previous researches were therefore invoked, and with them, acting sometimes singly and sometimes as conjugate mirrors, the curious and striking results, which I have now the honor to submit to the Society, were obtained.

Sulphuric ether, formic ether and acetic ether, being placed in bulbous flasks,† their vapors were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disc, so that the intermittent beam passed through the vapor, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapors which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapors. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapors of amylene, iodide of ethyl, iodide of methyl and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

* When the disc rotates the individual slits disappear, forming a hazy zone, through which objects are visible. Throwing by the clean hand, or, better still, by white paper, the beam back upon the disc, it appears to stand still, the slits forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add, that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colors of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colors disappear. The flashes also produced a giddiness sometimes intense enough to cause me to grasp the table to keep myself erect.

† I have employed flasks measuring from 8 inches to three-fourths of an inch in diameter. The smallest flask, which had a stem with a bore of about one-eighth of an inch in diameter, yielded better effects than the largest. Flasks from 2 to 3 inches in diameter yield good results. Ordinary test-tubes also answer well.

It is the vapor, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapor-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rock salt cell filled entirely with a volatile liquid, and subjected to the intermittent beam, produced no sound. This cell was circular, and closed at the top. Once, while operating with a highly athermanous substance, a distinct musical note was heard. On examining the cell, however, a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disc, and allowing its slits to act like the perforations of a syren.

Thus, as regards vapors, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam, it yielded a musical note, but so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid, the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas it gave birth to a musical note which, when the beam was in good condition and the bulb well-chosen, seemed as loud as that of an ordinary organ pipe.* We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorption and the intensity of the sound go hand-in-hand.

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*With conjugate mirrors the sounds with olefiant gas are readily obtained at a distance of twenty yards from the lamp. I hope to be able to make a candle flame effective in these experiments.

A soap bubble blown with nitrous oxide or olefiant gas, and exposed to the intermittent beam, produced no sound, no matter how its size might be varied. The pulses obviously expended themselves upon the flexible envelope, which transferred them to the air outside.

But a film thus impressionable to impulses on its interior surface must prove at least equally sensible to sonorous waves impinging on it from without. Hence, I inferred, the eminent suitability of soap bubbles for sound lenses. Placing a "sensitive flame" some feet distant from a small sounding reed, the pressure was so arranged that the flame burnt tranquilly. A bubble of nitrous oxide (sp. gr. 1·527) was then blown and placed in front of tl e reed. The flame immediately fell and roared, and continued agitated as long as the lens remained in position. A pendulous motion could be imparted to the bubble, so as to cause it to pass to and fro in front of the reed. The flame responded by alternately roaring and becoming tranquil to every swing of the bubble. Nitrous oxide is far better for this experiment than carbonic acid, which speedily ruins its envelope.

The pressure was altered so as to throw the flame, when the reed sounded, into violent agitation. A bubble blown with hydrogen (sp. gr. 0·069) being placed in front of the reed, the flame was immediately stilled. The ear answers instead of the flame.

In 1859 I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was therefore great. Placing a small quantity of liquid ammonia in one of the flasks, and warming the liquid slightly, the intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced. By the proper application of heat to a liquid the sound may be always intensified. The ordinary temperature, however, suffices in all the cases thus far referred to.

In this relation the vapor of water was that which interested me most, and as I could not hope that at ordinary temperatures it existed in sufficiant amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling point. Placed in the intermittent beam, I heard—I avow with delight—a powerful musical sound produced by the aqueous vapor.

Small wreaths of haze, produced by the partial condensation of the vapor in the upper and cooler air of the flask, were, however, visible in this experiment; and it was necessary to prove that this haze was not

the cause of the sound. The flask was, therefore, heated by a spirit-flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam of light then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapor, however, the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10°C., fully expecting that the sound would vanish at this temperature; but, notwithstanding the tenuity of the vapor, the sound extracted from it was not only distinct, but loud.

Three empty flasks, filled with ordinary air, were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit-lamp until all visible humidity had been removed, and afterwards urging dried air through them, on being placed in the intermittent beam the sound in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the athermancy and diathermancy of gases and vapors, and it cannot be otherwise than satisfactory to me to find that particular vapor, whose alleged deportment towards radiant heat has been so strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapor, we are prepared for the fact that an exceedingly small percentage of any highly athermanous gas diffused in air suffices to exalt the sounds. An accidental observation will illustrate this point. A flask was filled with coal-gas, and held bottom upwards in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal-gas. The flask was then placed upright, with its mouth open, upon a table, and permitted to remain their for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.*

* The method here described is, I doubt not, applicable to the detection of extremely small quantities of fire-damp in mines.

Transferring a small flask or a test-tube from a cold place to the intermittent beam, it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporization by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids and their vapors absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapor, would detach the effective rays, and thus quench the sounds. The experiment was made, and the conclusion verified. A layer of water, formic ether, sulphuric ether or acetic ether, one-eighth of an inch in thickness, rendered the transmitted beam powerless to produce any musical sound. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon, about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapors—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that with those gases which yielded a feeble sound the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds the displacement was prompt and forcible.

Further Experiments.—Since the handing in of the foregoing note, on the 3d of January, the experiments have been pushed forward; augmented acquaintance with the subject serving only to confirm my estimate of its interest and importance.

All the results described in my first note have been obtained in a very energetic form with a battery of 60 Grove's cells.

On the 4th of January I chose for my source of rays a powerful lime-light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a nearer approach to the source of rays. Tested with this new reflector the stronger vapors rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those of the lime-light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapors.

Abandoning the mirror and bringing the candle close to the rotating disc, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the rotating disc, produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds. Maintaining the flask in position behind the rotating disc, amusing alternations of sound and silence accompanied the alternate introduction and removal of the poker.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water.

As might be expected from the foregoing experiments, an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three the sounds, though enfeebled, were still distinct.

My neglect of aqueous vapor has led me for a time astray in 1859, but before publishing my results I had discovered my error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks, and permitted to remain in the laboratory from the 23d of December to the 4th of January. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due not to dry air, but to traces of aqueous vapor.

An ordinary bottle, containing sulphuric acid for laboratory purposes, being connected with the ear and placed in the intermittent beam, emitted a faint but distinct musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some

days, was placed in the beam; the dry air above the liquid proved absolutely silent.

On the evening of January the 7th, Professor Dewar handed me four flasks treated in the following manner. Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium, while the fourth contained a small quantity of phosphoric anhydride. They were closed with well-fitting india rubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last mentioned being the strongest. Tested again six hours later, the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid, instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with the sulphuric acid always enfeebles, and sometimes destroys the sound.

A bulb, less than a cubic inch in volume, and containing a little water, lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit-lamp the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb* is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapor. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapor.

The action of aqueous vapor being thus revealed, the necessity of thoroughly drying the flasks, when testing other substances, becomes obvious. The following plan has been found effective. Each flask is first heated in the flame of a spirit-lamp till every visible trace of internal moisture has disappeared, and it is afterwards raised to a temperature of about 400° C. While the flask is still hot a glass tube is introduced into it, and air freed from carbonic acid by caustic potash, and from aqueous vapor by sulphuric acid, is urged through the flask until it is cool. Connected with the ear-tube, and exposed immediately

* In such bulbs even bisulphide of carbon vapor may be so nursed as to produce sounds of considerable strength.

to the intermittent beam, the attention of the ear, if I may use the term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

In 1868, I determined the absorptions of a great number of liquids whose vapors I did not examine. My experiments having amply proved the parallelism of liquid and vaporous absorption, I held undoubtingly, twelve years ago, that the vapor of cyanide of ethyl and of acetic acid would prove powerfully absorbent. This conclusion is now easily tested. A small quantity of either of these substances, placed in a bulb a cubic inch in volume, warmed and exposed to the intermittent beam, emits a sound of extraordinary power.

I also tried to extract sounds from perfumes, which I had proved, in 1861, to be absorbers of radiant heat. I limit myself here to the vapors of patchouli and cassia, the former exercising a measured absorption of 30 and the latter an absorption of 109. Placed in dried flasks, and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than that of patchouli.

Many years ago, I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh-gas was of special interest. Professor Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask and permitted to displace the air within it. Exposed to the intermittent beam its sound was similar in power to that of marsh-gas.

The specific gravity of marsh-gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This, however, is not the case. After a considerable interval the film of this gas clinging to the interior surface of the flask was able to produce sounds of great power.

A small quantity of liquid bromine being poured into a well-dried flask, the brown vapor rapidly diffused itself in the air above the liquid. Placed in the intermittent beam a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapor. But my former experiments on this vapor were conducted with obscure

heat; whereas, in the present instance, I had to deal with the radiation from incandescent lime, whose heat is, in part, luminous. Now, the color of the bromine vapor proves it to be an energetic absorber of the luminous rays, and to them, when suddenly converted into thermometric heat in the body of the vapor, I thought the sound might be due.

Between the flask containing the bromine and the rotating disc I therefore placed an empty glass cell; the sounds continued. I then filled the cell with transparent bisulphide of carbon; the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light, while allowing the rays of heat free transmission; the sounds were immediately stilled.

Iodine vaporized by heat, in a small flask, yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might, indeed, have been foreseen that the rays transmitted by the iodine as a liquid, would also be transmitted by its vapor, and thus fail to be converted into sound.*

To complete the argument. While the flask containing the bromine vapor was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disc. There was no sensible abatement of the sounds with either bromine or iodine vapor.

In these experiments the rays from the lime-light were converged to a point a little beyond the rotating disc. In the next experiment they were rendered parallel by the mirror, and afterwards rendered convergent by a lens of ice. At the focus of the ice lense the sounds were extracted from both bromine and iodine vapor. Sounds were also produced after the beam had been sent through the alum solution and the ice-lense conjointly.

With a very rude arrangement I have been able to hear the sounds of the more active vapors at a distance of 100 feet from the source of rays.

Several vapors other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapors of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that even the

*I intentionally use this phraseology.

vapors of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

Note.—With some of the strongest sounds, which were audible when the ear-tube was entirely withdrawn from the ear, I tried to obtain the agitation of soap film. A glass tube, blown into a shape somewhat resembling a tobacco pipe, had its mouth closed by such a film, while its open stem was connected with a sounding flask. I did not succeed in producing any visible agitation. When the film was uniformly illuminated, or when it had become thin enough to produce iridescent colors, on holding a high-pitched tuning-fork near the open end of the stem the whole surface of the film was immediately covered with concentric rings, having the centre of the film for their centre. This belongs to the class of effects so vividly described by Mr. Sedley Taylor. A fork of the pitch of the sounding gas produced no visible effect upon the film.

THE MOON OF EARTH AND JUPITER.

By Pliny Earle Chase, LL.D.
Professor of Philosophy in Haverford College.

One of the most interesting chapters in Laplace's "Mecanique Celeste" is the one which describes tendencies to synchronism, among Jupiter's satellites, similar to those which are well known to watchmakers, in timekeepers which are suspended near each other. Some recent investigations in photodynamics have led me to the discovery of the following additional harmonies, which serve to connect Callisto, Jupiter's outer satellite, with the Earth as well as with Jupiter. They are due to the fact that Callisto represents the centre of gravity of Earth and Jupiter, when they are in conjunction.

1. Earth's accelerated rotation is to acceleration by condensation to a centre of linear oscillation as Jupiter's radius of accelerated rotation is to Callisto's radius of accelerated rotation. This relation is shown by the following proportion:

$$366{\cdot}2565 : 9 :: 4332{\cdot}5848^{\frac{2}{3}} : 16{\cdot}6891^{\frac{2}{3}}.$$

2. Jupiter's secular mean perihelion distance from Earth is to Callisto's mean distance from Jupiter as Jupiter's mass is to Earth's mass. This is shown in the following proportion:

$$3·978245 : ·012585 : : \frac{1}{1047·88} : \frac{1}{331245}.$$

This proportion furnishes data for the following approximation to the distance of the Sun.

An isochronous radius is the mean distance at which a satellite would revolve about the planet in the same time as the planet revolves about the Sun. The cube root of the quotient of Sun's mass by planet's mass, multiplied by the isochronous radius, is 3962·8 $\times \left(\frac{31558149 \text{ sec.}}{5073·6 \text{ sec.}}\right)^{\frac{2}{3}} = 1340291·5$ miles; $331245^{\frac{1}{3}} \times 1340291·5 =$ 92736000 miles.

3. Jupiter's secular perihelion radius vector is to Earth's semi-axis major as Callisto's semi-axis major is to Moon's semi-axis major.

4·886325 : 1 : : ·012585 × 92736000 : 238847.

Lockyer gives, for Moon's distance,			. .	238793
Chambers	"	"	" . .	238830
Searle *	"	"	" . .	238870
Newcomb	"	"	" . .	240300

4 The radius of incipient nebular condensation is to the radius of secondary condensation (Callisto's semi-axis major) as the velocity of light is to the velocity of infinite fall.

6·130992 : ·012585 : : 186282 miles : 382·378 miles.

The "radius of incipient nebular condensation" represents the photodynamic energy which determined the positions of Neptune, Jupiter and Earth, with respect to Sun, and which also determined gravitating velocity.

Haverford College, Feb. 8, 1881.

Steam Carriages.—A steam carriage has been used for some time in Berlin. The Leipzig *Gazette* mentions that another German city, Chemnitz, the manufacturing centre of Saxony, with a population of about 50,000, is also using a steam car for the transport of merchandise through the streets without the use of rails. In two months it made forty-four trips, carrying 184,395 kilos (406,521 lbs.), which were easily distributed in all parts of the city, on grades and curves as well as on levels, without causing any accident to vehicles or pedestrians.—*Chron. Industr.* C.

* On the authority of von Littrow.

The Saharan Sea.—Commandant Roudaire has finished the investigations, which were indicated by the commission of the French Academy, in relation to the filling of the Tunisian and Algerian chotts by the sea. His conclusions are entirely favorable to the project, and would lead to the establishment of an interior sea, 400 kilometres (248·55 miles) long and 1600 kilometres (994·2 miles) in circumference.—*Comptes Rendus.* C.

New Lubricants.—K. Drechsler mixes graphite thoroughly with the whites or yolks of eggs, dries the mixture, pulverizes it and scatters it upon the parts of machinery which move slowly. G. Lieckfeld mixes graphite with soluble glass, so as to make a stiff broth. The mass is spread upon worn surfaces, where it soon hardens and can be filed or turned, so as to restore the machinery to its original perfection. —*Dingler's Journal.* C.

Difference in Galvanic Batteries.—Fr. Exner gives the following reasons why a Daniell battery does not show the influence of free oxygen so evidently as a Smee: 1. a concentrated solution of blue vitriol absorbs considerably less oxygen than acidulated water; 2. the hydrogen, which rises from the decomposition of the water, is abundantly supplied with $CuSO_4$ for the reduction of the copper; 3. it is well known that a Daniell element, if it remains long open, possesses a somewhat greater electro-motive force than if it was kept closed. This is an indication that the difference between a Daniell and a Smee element is only quantitative and not qualitative.—*Wiedem. Ann.* C.

The Place of Boron in the Series of Elements.—A. Etard gives satisfactory reasons for substituting glucinium, in the tabular position which Mendelejeff has assigned to boron. He finds that by the action of zinc ethyl upon boric ether we get triethylbromine, which, by its properties and its composition, corresponds very completely with triethylphosphine. After comparing successively the properties of boron and the phosphorus groups of elements, he unhesitatingly places boron at the head of the vanadium group, very near the phosphorus group. The resemblance of these two groups has already been established by Deville and Troost for niobium and tantalum, and by Roscoe for vanadium. These bodies, like boron, give volatile chlorides and oxychlorides, the density of which has already been measured, and they exhibit various other evidences of similarity in their respective salts and acids.—*Comptes Rendus.* C.

Origin of Phenol in Animals.—Prof. R. Engel, in studying the transformations of phenol in the animal economy, finds that the animal economy itself is the product of the pancreatic digestion of albuminoid substances, and of the putrefaction of those substances in the intestines.—*Ann. de Chim. et de Phys.* C.

Spectroscopic Study of the Sun.—L. Thollon is carefully studying the sun's surface at the Paris observatory. He describes a number of interesting hydrogen explosions, resembling gigantic displays of fireworks, and sometimes remaining visible for two days. One of them was more than 200,000 miles in height, and explosions of from 25,000 to 50,000 miles in height are quite common..—*Comptes Rendus.* C.

Electric Discharges in Gases.—E. Wiedemann has investigated the thermal and optical phenomena of gases under the influence of electric discharges. He concludes that specific changes of quality and quantity in the electric spectrum of a gas must be represented by equal quantities of energy in each molecule, which are independent of the pressure of the gas and of the diameter of the tube.—*Wiedemann's Annalen.* C.

Accurate Longitudes.—Admiral Mouchez commends the accuracy of Messrs. Green and Davis, in their determinations, by the transatlantic telegraph, of some of the principal longitudes on the coast of Brazil. He was himself charged, in 1860, with the task of determining the hydrography of Brazil and La Plata. In the performance of his duties he made a large number of astronomical observations, which accorded so closely with previous records that he affirmed in his report that the longitudes were known with nearly as great accuracy as those of the great observatories of Europe. The French astronomers, however, discredited his results and adopted values about 30 seconds greater. The American observers have fully confirmed Mouchez's assertions, showing that his greatest error was only 2·34 seconds, and in one case the error was only five-eighths of a second. Errors of from 2 to 4 seconds have been recently detected in the longitudes of some of the European ports. These comparisons also show the great precision with which longitudes can be ascertained by chronometers, when their rates are properly verified at the end of every voyage.—*Comptes Rendus.* C.

Filtration by Spongy Iron.—Messrs. Easton and Anderson are testing the value of spongy iron upon a large scale in the filters of the water works at Antwerp. The water is first allowed to pass through a mixture of iron and gravel covered with sand, when it passes into a second basin, the bottom of which is covered with sand. The experimental results have been more satisfactory than those from ordinary filters, and there are no indications of any necessity for renewing the iron, which serves to oxidize the organic matters suspended in the water.—*Ann. des Ponts et Chauss.* C.

Fusion of Metals by Electricity.—M. Imbert describes Siemens' method of fusing large metallic masses by means of electricity. He uses a plumbago crucible, surrounded by a thick refractory wall, the cover being traversed by a carbon rod of 20 millimetres (·79 inch) diameter. This rod is suspended by one of the arms of a balance beam, the other arm carrying a cylinder of soft iron sliding freely in a solenoid and plunging into a liquid, in order to moderate the oscillations which might arise from sudden variations of current. In one experiment 500 grammes (1·102 lbs.) were melted into a compact ingot in 4½ minutes. In melting large quantities the electrical method is rather more than twice as costly as the ordinary furnace, but for the fusion of precious or refractory metals, for chemical purposes, and for other applications where the question of economy is secondary, the new method is very convenient and practical. In melting small quantities it may even prove economical.—*Ann. du Gen. Civ.* C.

Book Notices.

Manual of Cattle Feeding: A Treatise on the Laws of Animal Nutrition and the Chemistry of Feeding Stuffs in their Application to the Feeding of Farm Animals. With illustrations and an appendix of useful tables. By Henry P. Armsley, Ph.D., Chemist to the Connecticut Agricultural Experiment Station. 12mo. John Wiley & Son, New York.

This work belongs to the higher class of publications demanded by the rapid advance of agriculture. It treats not only of the best practice of feeding, but also of the theory, and gives us the results obtained at European experiment stations. The chemistry and the

physiology of the nutrition of domestic animals are clearly explained in order to enable the attentive reader "to adapt his practice to the varying conditions in which he may be placed; and more important still, to follow or take part in the advance of the science."

Part I of the work treats of the general laws of animal nutrition, in which not only are the values of the nitrogenous, non-nitrogenous and inorganic nutrients discussed, but the structure and functions of the organs of digestive circulation and respiration are briefly and very clearly described. The method of determining the nutritive effect of a ration and the part it plays in the formation of flesh and fat and in the production of work, are also well treated.

Part II is devoted to the feeding stuffs and their digestibility. In treating of the coarse fodders, and the method of curing them, ensilage is not forgotten, and the author's opinion of the process may perhaps be learned from the following quotations: "Ensilage of itself adds nothing to the value of the fodder submitted to it, but rather diminishes it. In the *sils* a sort of fermentation is carried on at the expense of the extractive matter of the fodder, resulting in the fermentation of various organic acids and volatile bodies and naturally diminishing the quantity of (non-nitrogenous) extract, and thereby increasing the percentage of all the other ingredients. Corn being a comparatively cheap crop, the loss of material during the fermentation (of the fodder) might be compensated by the improved quality of the residue." In the chapter on the concentrated fodders the following analyses of American linseed and cotton-seed cake are given, the samples having first been deprived of water.

	Ash.	Protein.	Crude fibre.	Non-nitrogenous extract.	Fat.
Linseed cake, per cent.,	7·13	32·48	9·70	37·66	13·03
Cotton-seed cake, "	7·33	46·17	7·98	19·98	18·54

But one sample of linseed oil-cake, from which the oil had been been extracted under the Simonin patent employed in this city, appears to be mentioned; that contained 3·15 per cent., while the protein was 39·92 per cent) Our author adds: "Cotton-seed cake, it will be seen above, is considerably richer in protein and fat, and poorer in (non-nitrogenous) extract than linseed cake, and must have correspondingly higher feeding value."

The feeding of farm animals is especially treated in Part III, of which the chapter on "feeding standards" is the first. By this term

is meant the quantities and proportions of the nutrients required for the several races of animals when the object is maintenance, fattening or work. Nutrients of a ration are divided into nitrogenous and non-nitrogenous, and the proportion of the former to the latter is called "the nutritive ratio." The ordinary day's work of a horse being taken as equal to the raising of 1,500,000 kilogrammes one metre high, the feeding standard is given in lbs. per 1000 lbs. live weight.

Feeding Standard Horse.

Digestible nitrogenous principles (protein), . .	1·8 lbs.
Digestible non-nitrogenous principles (carbhydrates and fat),	11·8 "
Nutritive ratio, ...	1 : 7
Total matter, freed from moisture. . .	22·5 lbs.

Tables are given showing the average composition and digestibility of the common feeding stuffs, and also the range of variation in this respect as far as observed. From these tables may be ascertained how much digestible matter is available in the feed we are using, and how much must be added to or taken from this ration in order to bring it to the feeding standard. On the subject of which it treats the book is far in advance of any other American publication we have read.

A. L. K.

BALDWIN LOCOMOTIVE WORKS. Illustrated Catalogue of Locomotives. Second edition. Royal 8vo. Philadelphia: 1881.

Of this catalogue it is impossible to speak in terms of too much praise. It gives a more varied and exact summary of the history of the locomotive than has heretofore appeared in any catalogue in this country, and more possibly than has appeared in all the other catalogues yet issued upon this subject. The whole is contained in a large 8vo volume of 152 pages, with eighteen photographs and eleven wood-cuts of their different types of locomotives; also sixteen wood-cuts descriptive of the historical part.

On page 96 will be found a cut of their "Single Fast Passenger Locomotive," similar to locomotive "5000," the 5000th of their build, which was originally intended for the Philadelphia & Reading Railroad Company, to run on the Bound Brook route, between Philadelphia and New York. It has made the distance, 89·4 miles, in 98 minutes, being at the rate of 54 miles an hour.

On page 68 also will be found a photograph of what is known as the "American" express passenger locomotive, there being more of

this type in use than any other style. One of the pattern known as 8—28C, having cylinders 17 x 22 inches and drivers 66 inches diameter, has pulled a train of 456,725 pounds (including locomotive and tender of 132,000 pounds), fifty-nine miles, counting one stop and one slow-down on drawbridge, in seventy-five minutes. This class is also largely in use for light freight trains. On page 103 is a cut of their celebrated ten-wheel freight locomotive known as 10E "Consolidation," which are now in extensive use for heavy freight trains on all the leading roads in America.

The first locomotive built in this country had but *one pair of driving wheels*, as in the "Old Ironsides," page 7 of catalogue. The experiments made with her, Mr. Baldwin says, "were eminently successful, realizing the sensation of a flight through the air of fifty or sixty miles an hour." The Old Ironsides, in its general arrangements, was a close copy of the English locomotives brought to this country in 1831, and up to 1840 was the leading type of "Fast Passenger Engine."

In 1842 the increase of business on railroads demanded more powerful locomotives, and it was found that for heavy freight business one pair of drivers was insufficient, and recourse had to be made to *coupling* an additional pair of wheels to secure the necessary adhesion so as to be able to pull the increased weight.

This catalogue is well written, and its description of the leading improvements in the construction of the locomotive, from its conception in America to the present day, will be found both interesting and instructive. In mechanical execution the volume leaves nothing to be desired, and it will be always preserved for the valuable information it contains. W. B. LeV.

Franklin Institute.

HALL OF THE INSTITUTE, March 16th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 102 members and 87 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and

announced, that at their last meeting 13 persons were elected members of the Institute; also that, by the resignation of Prof. Elihu Thomson, a vacancy existed in the Board. The President stated that filling this vacancy was the privileged business of the evening, and he therefore invited nominations. Prof. Wm. D. Marks and Mr. W. H. Thorne were placed in nomination, and tellers being appointed, they announced that Prof. Marks had received a majority of the votes cast, and he was thereupon declared a member of the Board for the unexpired term of Prof. Thomson.

The Secretary reported the following donations to the Library:

Airy—Mathematical Tracts. Cambridge, 1826.
Miller—Differential Calculus. Cambridge, 1837.
Bertrand—Analyse Mathematique. Paris, 1867.
Delaunay—Progresse de l'Astronomie. Paris, 1867.
Booth—Elliptic Integrals. London, 1851.
Nautical Almanac for 1870. London, 1866.
Frost and Wolstenholme—Solid Geometry. Cambridge, 1838.
Catalan—Theorèmes, etc., de Geometrie. Paris, 1865.
Salmon—Curves. Dublin, 1852.
Salmon—Algebra. Dublin, 1866.
Drew—Conic Sections. 1862.
Euclid's Geometry. Oxford, 1862.
Mulcahy's Geometry. Dublin, 1862.
Salmon's Geometry. Dublin, 1865.
Salmon's Conic Sections. London, 1863.
Godfrey's Astronomy. Cambridge, 1866.
Wilson's Geometry. 2 vols. Cambridge, 1868.
Bradley's Geometry. London.
Main's Astronomy. Cambridge, 1868.
Hann's Calculus. London, 1850.
Cheyne's Planetary Theory. Cambridge, 1862.
Godfrey's Lunar Theory. Cambridge, 1859.
Boole's Differential Equations. Cambridge, 1860.
Boole's Calculus of Finite Differences. Cambridge, 1860.
Cox's Integral Calculus. London, 1852.
Woolhouse—Differential Calculus. London.
Airy's Popular Astronomy. London, 1868.
Lockyer's Lesson's in Astronomy. London, 1868.
Ferrer's Trilinear Co-ordinates. London, 1866.
Taylor's Geometrical Conics. Cambridge, 1863.
Wolstenholme's Mathematical Problems. London, 1867.
Airy's Error of Observations. Cambridge, 1861.
Drew's Conic Sections. London, 1864.

Lagrange's Mecanique Analytique. Paris, 1853–5.
Parkinson's Treatise on Mechanics. Cambridge, 1863.
Tait and Steele's Dynamics. Cambridge, 1865.
Wurtz's Chemical Theory. London, 1869.
Airy—Atmospheric Vibrations. London, 1868.
Parkinson's Optics. London, 1866.
Regnault and Strecker's Chemistry. Braunschweig, 1869.
Newth's Natural Philosophy. London, 1868.
Quet—Electricity, Magnetism, etc. Paris, 1867.
Airy's Optics. London, 1866.
Bertin's Thermodynamics. Paris, 1867.
Routh—Dynamics of Rigid Bodies. London, 1868.
Desain's Theorie de la Chaleur. Paris, 1868.
Airy's Gravitation. London, 1834.
Whewell's Dynamics. Cambridge, 1832.
Frost's Newton's Principia. Cambridge, 1863.
Whewell's Newton's Principia in Latin. London, 1846.
Evans' Newton's Principia. Cambridge, 1855.
Weinhold's Vorschule der Experimentalphysik. Leipzig, 1871.
Lloyd's Wave Theory of Light. London, 1857.
Hirn's Thermodynamics. Paris, 1868.
Besant's Hydromechanics. Cambridge, 1867.
Helmholtz's Optics. Leipzig, 1867.
Lunn's Motion. Cambridge, 1859.
Thorpe's Chemical Problems. London, 1870.
Besant's Hydrostatics. Cambridge, 1867.

Presented by Mr. Alexander E. Outerbridge, United States Mint, Philadelphia.

History of Coaches. By G. A. Thrupp. London, 1877.
From the Author.

Mr. T. Mellon Rogers read the paper announced for the evening, on "The Concentration of Ores," and also exhibited his new Swing Gate Concentrator. It is a novel apparatus for washing and concentrating gold, silver and other metals in ores of low grades. The machine consists of a water tank, divided into two compartments, in one of which is suspended a box with screen for holding the ore, while the other takes the waste. In the bottom of each are hoppers with eduction pipes. In the main compartment is a vibrating gate, provided with valves, and moved by a rod and adjustable eccentric, driven by steam power. The forward swing of the gate moves the large body of water in advance of it upward through the sieve, while at the same time a partial vacuum is produced in the smaller body of water

behind the gate, thus operating the valves which are opened and closed by variations in the pressure of the body of water on either side of them. Mr. Rogers exhibited the machine at work, and by the aid of drawings on the screen explained its operation. He said that there were many low grade ores, particularly in North and South Carolina and Virginia, that could not be profitably worked without concentration. If we could get a liquid of such density as to allow the ore to sink and the gangue to float, concentration would be very simple; but, as we have not, we must employ the best available substitute, which is water. Very rude apparatus is generally employed for concentrating the ores, and is really efficient, since the operator can vary the degree of agitation to be given it according to the ore. The apparatus devised by Mr. Rogers is designed to effect the same purpose by the use of an eccentric, readily adjustable to give changes of speed, by which to treat fine or coarse ores. The machine was exhibited in operation concentrating an ore containing argentiferous galena and gold sulphuret. Mr. Rogers also exhibited samples of concentrations made by him. In answer to questions he said that some of these machines were in use; that with meshes of from forty to sixty, eight tons could be concentrated in a day, and with larger meshes from eight to twelve tons, and that one important feature was the use of screws for leveling the box in which the ore is placed.

Mr. Tatham asked what advantage there was in the use of a swinging gate over an ordinary plunger.

Mr. Rogers replied that the swinging gate put the greatest agitation of the water where it would do the most good, and prevented a washing over and waste of ore that would result from a uniform raising of the water through the meshes.

The Secretary called attention to the system of underground telegraphy, invented by Restore B. Lamb and owned by the National Subterranean Electric Company. Terra cotta blocks, through which there are a number of small holes, glazed inside and out, are used. The pipes or blocks are joined together, much like sewer pipes; and cemented, the holes being continuous. Cables of wires, enclosed in rubber tubes, are carried through the holes. At convenient distances along the route are masonry chambers underground, entered from a manhole, in which the sections of tubular blocks terminate. In these the connections between two sections and repairs are made, the wires

or cables requiring repair being drawn through into the chamber, and then replaced. The wires are amply protected from the weather, and thoroughly isolated from each other, and, having once been laid, the streets through which they pass do not have to be disturbed to add additional wires as required, or to make repairs. Satisfactory experiments with the system have been made in Camden, where the "blocks" have been laid through swampy "made" ground for several months.

Several inquiries were made about the manner of laying the wires, the practical operation of the system, etc.

Mr. Patrick said there was some misapprehension apparently about the way in which the wires were drawn through the tubes. As he understood it the blocks were laid first with single leading wires through the several holes with which to draw through the cables from section to section and replace them. It is therefore not necessary to put in the whole number of wires which the blocks could carry at once; the number can be increased at will without disturbing the streets. The wires laid can be used for telephones, telegraphic or electric lighting.

Fleischmann's improved medical battery, which was shown, has a peculiar interrupter of the current by which alternate contraction and relaxation of the muscles may be obtained when the battery is used for medical purposes. The interrupter is a horizontal swinging lever—a flat spring about five inches long, very flexible at the secured end and having a weighted armature on the other end to be attracted by the electro-magnet. As it swings to and fro horizontal contact is made and broken by the spring. The pulsations are regular and can be varied from 60 to 300 per minute. It requires very small attractive power in an electro-magnet to keep it in motion. The battery also has an interrupter which may be used by itself. A modified Grenet battery is used.

T. Angell's portable vacuum steam heater, made by James P. Wood & Co., was shown by model. In the hot air chamber are a number of tubes closed at both ends and exhausted of air, but containing a small quantity of water. Each tube is independent of the others, and each is hermetically closed. A small part of the tube extends into the fire-chamber, the greater part being in the hot air chamber, which has the usual induction and eduction air flues.

The American star bicycle was exhibited by the manufacturers, Messrs. Pressly and Crowell. It was invented by G. W. Pressly, and

has the smaller guiding wheel in the front, to prevent "headers." The wheel is strongly constructed and is propelled by a system of levers and clutches instead of by crank.

Crocker's reversible self-packing and cleaning filter can be thoroughly cleansed without removing it from the faucet by simply reversing the ball, and turning on the water. The filter acts by allowing the liquid to pass through two fine metal strainers, and a body of animal charcoal, which can be easily renewed at any time. If desired, a free passage of the water in a groove around the ball, without filtering, can be obtained by a slight turning of the handle attached to the ball. Dessau's diamond drill, the Burlington note, draft and receipt file, E. J. Smith's bill and paper file, and Fleischmann's toy electric motor were also shown by the Secretary.

A colored plan of the Library and Reading Room was exhibited, prepared by Mr. E. Hiltebrand, Librarian of the Institute, for the benefit of members consulting books, and gives the exact position of all the cases, their number and the general subject of their contents, so that any one can readily find the proper case without the aid of the officer in charge.

A communication, inviting the co-operation of the Franklin Institute in the preparations now being made for the celebration of the Bi-Centennial of the landing of William Penn, was read. A committee consisting of Charles H. Banes, Coleman Sellers and Dr. Isaac Norris was named to represent the Institute in the matter.

A resolution for the appointment of a committee to confer with other bodies on the subject of the removal of a part of the Permanent Centennial building to Washington Square was offered by Mr. Eldridge.

Mr. Weaver said that he had been informed that the work of improving Washington Square was about to be commenced, and in reply Mr Mitchell said that he believed Washington Square could not be used for any other purpose than that of a public square.

Mr. Eldridge said he could not see that any harm could come from the appointment of the committee which was only to be prepared to get information on the subject if the movement should amount to anything. If the building could not be moved to Washington square the committee would have nothing to do.

Mr. Weaver moved to indefinitely postpone the resolution, which was agreed to, and the Institute then adjourned.

ISAAC NORRIS, M.D., *Secretary.*

JOURNAL

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FOR THE PROMOTION OF THE MECHANIC ARTS.

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ON THE RATIO OF EXPANSION AT MAXIMUM EFFICIENCY.

By R. H. Thurston.

Read before the American Society of Mechanical Engineers, May, 1881.

In all heat-engines the method of transformation of heat-energy into useful mechanical work is the following:

A certain mass of the working fluid is heated from a temperature which is usually not far from that of the atmosphere up to some higher temperature, T. This is accompanied by a definite increase of volume, or of pressure, or of both, and in the case of liquids by a change of physical state after passing a certain point which is variable, but definite for each pressure; this latter temperature is the boiling point and the change is that known as vaporization. Evaporation being complete, the mass is expanded until it has attained a certain larger volume, v_2, the magnitude of which is r times that of the initial volume, v_1, with which expansion began. We thus have the "*rate of expansion*," $r = \frac{v_2}{v_1}$.

When expansion is complete, the whole volume, v_2, of steam or gas at the pressure p_2 is rejected from the cylinder into a condenser or into the atmosphere, and the piston which it has impelled through the

total volume, v_2, returns to the starting point, resisted by the "*back-pressure*," p_3, of the condenser or of the atmosphere. During the latter operation all heat which has not been transformed into work is rejected, and an additional amount is expended, which is equivalent to the work done by the piston upon the fluid during its expulsion. This operation is also similar in result to the following: Instead of exhausting the working fluid as described, abstract heat from it at the maximum volume, v_2, until its pressure becomes equal to the back-pressure; then compress at the constant pressure p_3 until the fluid is restored to its original volume at the pressure p_3, which volume, in the case of the steam engine, may be neglected. In the case of the gas engine or hot air engine this volume is that of the working fluid at initial temperature and pressure.

This process is thus graphically represented: In Figure 1, the fluid, initially in the state measured by the pressure $a\ E$ or $a'\ E'$ and volume $O\ a$ or $O\ a'$, is heated, sometimes at constant volume, as $O\ a$, and sometimes with compression, as from $O\ a'$ to a higher temperature, the pressure and volume varying as shown by $E\ A$ or by $E'\ A$. Heated next at constant pressure or at constant temperature, the mass expands, doing work, to B or to B'. At this point, v_1, p_1, the supply of heat ceases and the fluid expands "adiabatically," transforming into mechanical energy all the heat demanded as equivalent to the work measured by the area $b\ B\ c\ C$, and drawing upon its own stock of heat to supply this demand. At the end of this stage the fluid has a lower temperature and a pressure and a volume, $c\ C$, $O\ c$ (p_2, v_2), determined by that temperature and the value of $r = \frac{v_2}{v_1}$, and which are indicated by the location of the point C.

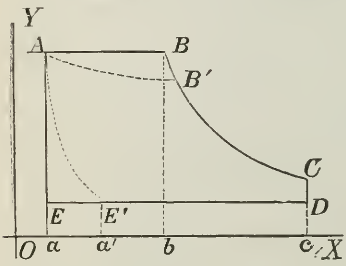

Fig. 1.

Rejecting heat at constant volume, v_2, pressure falls to D, p_3, and then rejection of heat continuing at constant pressure, p_3, the volume is reduced to that with which it started.

The *total* or *gross* work done is, in gas engines, measured by the area $A\ B\ C\ c\ a\ A$, in steam and vapor engines by this area increased by a very considerable amount—the measure of internal, of molecular, work which cannot appear on the indicator diagram.

The net work done is measured by the area included in the indicator diagram $A\ B\ C\ D\ E\ A$. This work is the equivalent of all heat transformed into mechanical work or energy. *The efficiency of the fluid* is the ratio of *net* work done to total heat received by the fluid, and is a maximum when the area $A\ B\ C\ D\ E$ is a maximum, assuming the *rate of expansion* alone to vary. It is evident that this maximum is determined, therefore, by the conditions which make the area $b\ B\ c\ C$ a maximum, which conditions are very simple in the hot air engine, and are easily expressed, while in the steam and in vapor engines they are very difficult of determination and expression in consequence of their extreme variability.

But the efficiency of the fluid is but one factor in the determination of the rate of expansion for maximum economy. The heat in the fluid is compelled to do its work, not simply through that fluid as a transmitting mechanism, but also through a machine which, as an apparatus intended to imprison and direct so subtle and elusive a form of energy as heat, is extremely imperfect, and which has the additional and very serious defect of being itself cumbersome and difficult to start and to keep in motion without considerable loss of power within itself.

The useful work of the machine is that which it transmits beyond its own boundaries to other mechanisms, and this is a maximum at that rate of expansion which gives energy to the machinery of transmission beyond the engine at least cost in heat expended. This *efficiency of the system* is therefore the product of two factors, the *efficiency of the fluid* and the *efficiency of the engine* consideredas a piece of mechanism.

(1) Taking first the purely ideal case in which the mechanism is assumed to be perfect and the rate of expansion the only variable element, we may by examining Figure 2 see at once what should be the value of that rate.

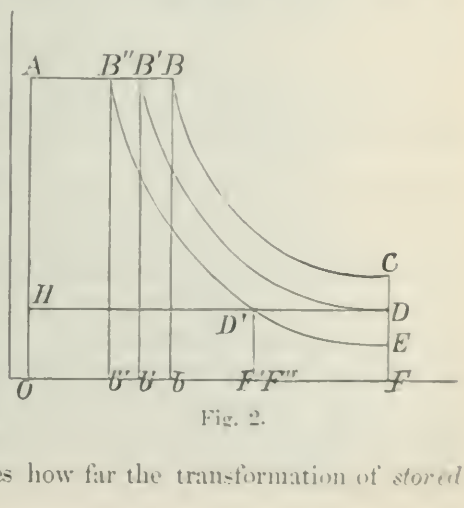

Fig. 2.

It is obvious that the rate of expansion simply determines how far the transformation of *stored*

heat-energy existing at B shall be continued by the development of work during the expansion of the working fluid. It is equally obvious that this expansion shall continue until the gain of work by further expansion is more than balanced by losses avoidable by termination of that process.

First: Where the only loss is due to a fixed back-pressure, $FD = p_3$, it is seen that, were expansion to cease at C, the work which would have been done had the expansion line $B\ C$ extended to the right, beyond C is lost, and that the counterwork of back pressure beyond that point is gained; but the former exceeds the latter and the net result is a loss by incomplete expansion. On the other hand, were the rate of expansion increased so that the expansion line becomes $B''\ E$, the back-pressure line is reached at D'; beyond this point, we note a gain of work done usefully, which is measured by the area $D'\ E\ F\ F'\ D'$, while a loss accrues by back-pressure measured by $D'DFF'D'$. We thus again meet with a net loss which is represented by $D'DED'$, and expansion has evidently been carried too far. Making the value of $r = \frac{v_2}{v_1}$ such that expansion reaches the back-pressure line at D and p_2 becomes equal to p_3, we meet with neither kind of loss, and it follows that expansion should in this ideal case be continued until the expansion line meets the back-pressure line.

This may be readily shown by other methods. It was shown, nearly two generations ago, by Sadi Carnot, that maximum efficiency of *fluid* is attained when expanding between the widest possible limits of temperature.

It is now well known, and it is shown by every elementary treatise on physics, or mechanics, or thermodynamics, and on heat-engines, that the efficiency of the *fluid* in any heat engines is measured by the expression $\frac{T_1 - T_2}{T_1}$ in which T_1 and T_2 are the temperatures of reception and rejection of heat measured from the "absolute" zero. But this maximum range of temperature corresponds to the maximum attainable range of pressure, and, the upper limit being fixed, this range is determined by the value of r and is a maximum when $p_2 = p_3$ and expansion continues to the back-pressure line.

A general analytical demonstration is obtained in the following manner: Given p_1, v_1, v_2, p_3 to find the value of *rate of expansion* r

to make the net work done a maximum. This work, $A\ B\ C\ D\ E$, Fig. 1, is measured by

$$W_a = p_1 v_1 + \int_{v_1}^{v_2} p\ dv - p_3\ v_2; \tag{1}$$

it is a maximum when the variable part $\int_{v_1}^{v_2} p\ dv - p_3$ is a maximum.

The method of variation of p with variation of v is determined by various conditions which need not be discussed here and which do not affect our analysis. Let this relation be such that we may write, as experiment indicates that we may do with practically close approximation,

$$p_1\ v_1^n = p_2\ v_2^n.$$

Thus we have

$$W_n = p_1\ v_1 + \int_{v_1}^{v_2} p\ dv - p_3\ v_2;$$

$$= p_1\ v_1 + \frac{p_1 v_1 - p_2 v_2}{n-1} - p_3\ v_2; \tag{2}$$

or, where $n = 1$,

$$W_a = p_1\ v_1\ (1 + \log_e r) - p_3\ v_2 \tag{3}$$

Determining the maximum for the first and usual case, we get

$$\frac{d\,W_n}{d\ r} = d\ \left(p_1\ v_1 + \frac{p_1 v_1 - p_1 v_1 r^{1-n}}{n-1} - p_3\ r\ v_1\right)\ \frac{1}{dr} = 0;$$

whence
$$r = \left(\frac{p_1\ v_1}{p_3\ v_1}\right)^{\frac{1}{n}} = \left(\frac{p_1\ v_1}{p_2\ v_1}\right)^{\frac{1}{n}} = \frac{v_2}{v_1} \tag{4}$$

Hence $p_3 = p_2$ and the rate of expansion for maximum efficiency is that which makes the terminal direct pressure equal to the pressure resisting the motion of the piston.

This analysis must be modified when the expansion line is an equilateral hyperbola, in which case we have $n = 1$ and $p_1\ v_1 = p_2\ v_2$. This case is often assumed in the theory of gas and air engines, as it is that of isothermal expansion; but it is probably rarely observed in actual practice, and perhaps never occurs in steam and vapor engines. In simple calculations of work, however, the assumption does not usually lead to serious error,* and, so expanding the working

*See a neat demonstration of this case by Professor Marks in the JOUR. FRANKLIN INSTITUTE, June, 1880.

fluid, the energy exerted by it up to the point of cut-off is equal to the lost work due to back pressure; the net work done is measured by the total area under the expansion line of the indicator diagram, and the efficiency is proportional to log. r.

Thus we have

$$W_1 = p_1\, v_1\, (1 + \log_e r) - p_3\, r\, v_1$$

$$\frac{d\, W_1}{d\, r} = 0 = \frac{p_1\ v_1}{r} - p_3\, v_1$$

$$r = \frac{p_1}{p_3} = \frac{v_2}{v_1} = \frac{p_1}{p_2}; \qquad (5)$$

whence we again find $p_2 = p_3$.

(2) But in all real engines we have a resistance to the motion produced by the expanding fluid, which is composed of two parts, an actual back pressure on the piston $p_b = p_3$, as in the ideal case above, and a resistance due to friction of engine, including its pumps and attachments. It is evident that, as the latter, p_f, like the back pressure, p_b, is a constant source of lost work, we must terminate the expansion as soon as this source of loss produces a greater loss of power or of work than is gained by further expansion. In fact, given a certain value for the sum of these resistances, $R = p_b + p_f$, it is obvious that we may consider the whole as back pressure, if we choose, and that it is a matter of indifference, so far as the determination of the rate of expansion is concerned, what are their individual magnitudes.

To determine $R = p_b + p_f$, the sum of resistances due to back pressure, p_b, and to the frictional and other resistances—as of pumps, etc.—denoted by p_f, take an indicator card from the engine unloaded. Its mean pressure measures the friction, p_f, of the unloaded engine, and this, increased by a fraction of the pressure added by the load,* is the value of p_f. Or, still better, determine the indicated and the dynamometric power of the engine simultaneously; their difference is lost work, and the value of p_f, corresponding to that work, is that required.

Hence, for actual engines, where no other cause of loss exists to any appreciable extent, as in some types of air engines, we may write

$$W_b = p_1\, v_1 + \frac{p_1 v_1 - p_2 v_2}{n-1} - (p_3 + p_f)\, v_2 \qquad (5)$$

* Experiments made at the Novelty Works for the Navy Department, 1865–67, and others made later, do not exhibit this increase.

and by the process already outlined we obtain a maximum and deduce

$$p_2 = p_3 + p_f \tag{6}$$

Hence: *Where the only variable loss is due to back pressure and to friction of the engine, the rate of expansion should be such as to cause expansion to the mean pressure line of the engine diagram taken without load;* the useful work is, as before, the gross work done during expansion, and, thus adjusted, the net useful work and the efficiency are nearly proportional to log. r.

These conclusions may be taken practically without qualification for all actual engines in which the working fluid is gaseous and subject to little direct loss of heat.

(3) For steam and other vapor engines still further and still greater modification is necessary, since in such engines the departure from the ideal conditions first assumed is not only greater than in gas and air engines, but is so great as, in most cases, to lead to radically different rates of expansion. Even in the gas engines, the action of the working fluid, as assumed above, is very greatly modified by such variations from the ideal conditions as are here referred to.

These engines—steam and vapor—are impelled by a fluid, which is a vastly better receiver and transmitter of heat than the permanent gases. Steam takes up and loses heat, in the processes of formation and of condensation, with extreme rapidity. The working fluid, in all steam engines, is readily condensible, and exchanges heat with the metallic surfaces of the working cylinder with the greatest freedom. It is usually more or less wet, and its humidity is subject to rapid and extreme variation in the course of the movement of the piston.

Explosive and other gas engines are impelled by a mixture of hot gaseous and vaporous products of combustion, of which the latter portion is, like the working fluid in the steam and other vapor engines, subject to rapid and considerable changes of thermal state. Enclosed, usually, in a chamber the sides of which are kept cool by a water-jacket, enormous quantities of heat are lost as expansion proceeds, and the efficiency of the machine is correspondingly diminished, and the economical rate of expansion is altered by the increased losses which accompany the higher rates. It is easily seen that, should these losses increase in a high ratio, with large rates of expansion, a point will be reached, and may be reached quickly, at which any greater expansion will result in a loss exceeding in amount the work gained by the extension of the expansion line. This point may be reached, and pro-

bably often is reached, long before attaining the limit set by the value of the resistances already studied. In such a case, we may call this limit that of the *virtual back pressure due to condensation,* and may designate it as p_c.

(4) In the steam engine a still more complicated set of phenomena is to be met with, and the result of their action is similar to that just described.

Suppose steam to enter the steam cylinder perfectly dry, and to expand *adiabatically.** As expansion progresses, after the closing of the steam valve by the expansion gear, the work done by the working fluid results in the transformation of so much heat into mechanical energy—which heat can now only be obtained by drawing upon the stock contained in the steam itself—that a part of the steam becomes liquefied.

This fact was shown by Rankine and by Clausius, by the study of the thermodynamics of the case; but it can easily and satisfactorily be shown by any student or engineer who will take the trouble to calculate the "total heats" of steam at the pressures found at the beginning and at the end of the expansion line. It will be found that the work done during the expansion is greater than the mechanical equivalent of this difference, and it follows that a part of it must have been done at the expense of the heat of evaporation of the expanding mass. This must be the case up to a limit at which the sensible heat of steam is just equal to the latent heat of the mass when indefinitely expanded at the absolute zero, and is about $\frac{1109550}{772} = 1437°$ $\pm$ absolute, a temperature only attained at the red heat.

Steam, therefore, condenses in the steam cylinder unless, by superheating or by the use of an efficient jacket, considerable heat is supplied it during expansion or before. This amount is, however, insignificant in comparison with direct losses of heat; it can probably never approach ten per cent. of the heat supplied, and is more likely, usually, to be a very much smaller figure, perhaps two or two and a half per cent. for average cases.

Initial condensation and later re-evaporation of steam in the steam engine, and initial condensation without subsequent re-evaporation, in gas engines, give rise to losses that are both absolutely and relatively very great wherever the range of temperature during expansion is very considerable, and especially with low back pressure.

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* Without receiving or losing heat by exchange with surrounding surfaces.

The steam passing out of the exhaust ports to the condenser or into the atmosphere is moist and heavy with the water of condensation, and is a good conductor of heat as well as a very greedy absorbent. It sweeps out of the cylinder large quantities of heat abstracted from the inner surfaces of the cylinder, leaving those surfaces comparatively cold and wet with a chilling dew. The entering steam meets these cold metallic and liquid masses and is condensed in sufficient quantity to reheat them to the temperature of prime steam. As the piston moves forward it uncovers new surfaces, and condensation continues until, sometimes, a large fraction of the steam supplied lies in the cylinder or floats in the uncondensed steam as water and mist. Toward the end of expansion, and especially during exhaust, re-evaporation occurs at lower pressures and to a similarly serious extent. Thus heat is constantly transferred from the steam to the exhaust side, and, doing almost no work, is wasted, and the efficiency of the engine and the cost of fuel are greatly affected. The heat thus lost frequently amounts to 25 per cent. of the total supplied, and has been known not infrequently to amount to 50 per cent. In one case noted by the writer initial condensation was as high, *at least*, as 80 per cent.*

Since loss from this cause has been found to be so great, and to increase so rapidly with increased expansion, that it practically often sets an early limit to the economical increase of the rate of expansion, it is evident that we may determine a point such that, expansion being carried beyond it, the losses due this cause will exceed the gain of work done by the expanding fluid, precisely as in the cases already cited. Measuring the loss at such a point, we may determine the equivalent in foot-pounds of work, and thence deduce the magnitude of a new equivalent "*virtual* back pressure," $p_v = p_c$, which, if actually existing as such pressure, would similarly limit expansion.

* The extent of the loss from this cause is very seldom realized by engineers, and still less by unpractical writers on the theory of the steam engine. Even Rankine, the greatest of all known writers, seems to have failed to detect an initial condensation of 26 per cent, shown apparently by the value obtained for the index in his formula for the adiabatic expansion of steam $\left(p\ v^{\frac{10}{9}} = \text{constant}\right)$. He seems to check the discrepancy introduced into his analysis, amounting to a loss of about one-fourth of all heat supplied, by under-estimating the efficiency of the boilers, crediting them with but 0·54, where a low estimate, in the opinion of the writer, would be 0·65, and a fair figure would be 0·68 or 0·70 for the cases taken. The writer has never known the efficiency given in those estimates to be attained with boilers of such low value.

This value inserted in the formula representing the work of the steam would give a measure of the rate of expansion of maximum efficiency.

We should have, assuming no other losses:

$$W_c = p_1 \, v_1 + \frac{p_1 v_1 - p_2 v_2}{n-1} - p_c \, v_2 \qquad (7)$$

and should expand until $p_2 = p_c$, and make

$$r = \frac{v_2}{v_1} = \left(\frac{p_1}{p_c}\right)^{\frac{1}{n}}.$$

Thus, for all cases, whether of expansion of steam, of air, or of the products of combustion in explosive gas engines, we may determine for each case a certain "*virtual back pressure,*" which we may call p_v, by which to identify a point beyond which continued expansion leads to a loss of heat, or of work, or of both combined, that is greater than the gain by work done* in that additional expansion, and may write generally:

$$W_v = p_1 \, v_1 + \frac{p_1 v_1 - p_1 v_1 r^{1-n}}{n-1} - p_1 \, v_1 \, r^{1-n} \qquad (8)$$

$$r = \left(\frac{p_1}{p_v}\right)^{\frac{1}{n}}.$$

Where, as in the case in which air expands isothermally, the expansion line is an equilateral hyperbola, it is seen that the loss by the virtual back pressure is always equal to the work done in the engine up to the point of cut-off; for $p_1 \, v_1 = p_v \, v_2$.

The net work done is always, for such expansion,

$$W_n = p_1 \, v_1 \log._e r = p_1 \, v_1 \log._e \frac{p_1}{p_v}.$$

In general, for the *net* work shown on the card as $p_2 = p_v = p_1 \, r^{-n}$; $p_v \, v_v = p_1 \, v_1 \, r^{1-n}$; $p_v \, r^n = p_1$, we get for net work:

$$W_n = \frac{n}{n-1} \, p_1 \, v_1 \, \frac{r^{n-1} - 1}{r^{n-1}} = \frac{n}{n-1} \, p_1 \, v_1 \left[1 - \left(\frac{p_1}{p_v}\right)^{\frac{1-n}{n}}\right]$$

$$= \frac{n}{n-1} \, p_1 \, v_1 \left(1 - \frac{1}{r^{n-1}}\right). \qquad (9)$$

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* See "The Limitations of the Steam Engine," by Prof. Marks; JOURNAL FRANKLIN INSTITUTE, August, 1880. The use of the term "virtual back-pressure" is not logically correct, as the two methods of loss are quite different; but the writer has not yet found a more satisfactory term to take its place.

In the effort to determine the value of $p_v = p_r$ for this last method of loss of efficiency, we meet with great difficulties. The loss from initial condensation and later re-evaporation is the most serious of all those losses which in expansive engines are in any degree due to defects of the machine as a machine, and they are among those which are controllable to a considerable extent by the engineer. No two engines, however, ever exhibit them in the same degree, and modifying conditions are so numerous and so potent that the result of the most painstaking efforts to classify and to formulate them are likely to be exceedingly unsatisfactory.

This loss is proportionally greater as the range of temperature during expansion is greater; it is increased by slow speed of engine,* by reduction of the real back pressure, by increase in size of engine for a given amount of work done, by increase in conductivity of the surfaces of the working cylinder, and especially by wetness of steam. It is reduced by low rates of expansion, by increasing back pressures by reducing initial pressures, by increasing speed of engine and by special expedients, as steam-jacketing, superheating and the division of the expansion between two or more cylinders, as in "compound" or double-cylinder engines. Even increasing compression may reduce this loss and thus give a higher steam-line and an altered expansion-line.

The waste becomes the less, when the sides of cylinders only are jacketed, the smaller their diameter; it is lessened, when both heads and sides are jacketed, by increasing diameters, volumes being in both cases equal. With superheated steam, and where there is little initial condensation to be anticipated, the shape of cylinder is determined by the minimum ratio of volume to *internal* superfices, *i. e.*, $\frac{\text{diam.}}{\text{length}} = \frac{1}{2}$, except—as is often, if not usually, the case—when it is controlled by commercial considerations. The surfaces of the piston must evidently be included, since the principal losses—those due to initial condensation and to re-evaporation—occur upon those surfaces.

* With speeds so low that the range of temperature of cylinder surfaces is not restricted, the total weight of steam condensed is probably constant, and the loss becomes inversely as the speed of piston, or as the weight of steam passed through the engine. For fairly high and for very high speeds the writer takes this loss proportion to the reciprocal of the square root of the speed. See records of U. S. expansion experiments.

In general, we may say that the efficiency of an engine is some function of Δt, V, P and A; but the difference of temperature, Δt, is a function of pressures and time of exposure; the speed, V, determines time and exposure, and the area of surface exposed, A, is a function of volume, per unit of weight of steam, and of shape of cylinder. All of the conditions are so involved and interdependent that the simple approximate expressions to be presently given may be found preferable to any exact formula, even were it possible to devise them satisfactorily, and as these simple expressions yield, all things considered, very fair results, we may be fully justified in their use until extended and exact investigations shall yield better.

In gas engines the waste is decreased in those in which the working fluid meets only non-conducting surfaces, while it amounts as a minimum to 60 or 70 per cent. in some slow-running water-jacketed cylinders.

Again, we find some interesting compensations. The difference in back pressure between non-condensing and condensing engines is productive of such a wide difference in the range of temperatures worked through in usual cases, that the writer has been accustomed to consider the compensation so complete as to justify the assumption that the value of *this* "*virtual* back pressure" may be assumed to be independent of the magnitude of the actual back pressure, and to be determined solely by other conditions above noted. In steam-jacketed engines the efficiency of the steam-jacket is reduced by high speed, while the losses that it is designed to check are rendered less by the reduced effect of other causes of variation of the amount of initial condensation, and, while this compensation is by no means complete, the error introduced by the assumption that it is so may perhaps be neglected in presence of so many other and such complicated causes of irregularity of action. Our approximation must be anything but close at best. The exact expression would probably involve the Newton law of cooling and values of differences of temperatures, deduced from Rankine's formula: $\log. p = A - \frac{B}{T} - \frac{C}{T^2}$.

The best that the writer has been able to do in this direction, as yet, is to make simple and roughly approximate expressions for values of $p_t = p_v$, the proper terminal pressure, which, while widely departed from in many cases, may fairly represent average practice, and serve

as a guide to the designer and engineer until something better can be done, thus:

For the common unjacketed steam engine take the limit for p_v that determined by back pressure solely up to pressures of 6 or perhaps 7 atmospheres.

Then the terminal pressure should be, nearly, for non-condensing engines $p_t = p_v = p_b + p_f = 14{\cdot}7 + 3{\cdot}3 + 2 = 20$ lbs. per sq. inch (1·4 kilos. per sq. centimetre = 1·14 atmos.),* and we should expand to that pressure.

For condensing engines and for non-condensing engines under very high pressures, the limit is fixed by the extent of the losses of heat just described. For the common unjacketed engine at moderate speed the writer has been accustomed to assume for the rate of expansion giving maximum economy in the common single cylindered unjacketed engine, working at fair speed, $r_e < \frac{1}{2}\sqrt{P}$; for high-speed engines of best proportions and for compound engines, $r_e < \frac{3}{4}\sqrt{P}$ and $r_e < \sqrt{P}$ respectively. Where more variable conditions must be considered, he has written, for unjacketed cylinders or for ineffectively jacketed engines,

$$r_e = \frac{1}{p_v} = \frac{1}{p_c} = 0{\cdot}02\sqrt{SNP_1} = 0{\cdot}13\sqrt{S_m N P_m}, \text{ nearly;}\dagger$$

$$p_t = p_v = p_c = \frac{50}{\sqrt{SNP_1}} = \frac{8}{\sqrt{S_m N P_m}}, \text{ nearly;}$$

where S is the speed of piston, N the number of cylinders when the engine is "compound," P_1 and P_m are the initial pressures per square inch and per square centimetre, and p_t is the terminal pressure.

For well-jacketed engines we may take, roughly,

$$r_e = \frac{1}{p_v} = \frac{1}{p_c} = 0{\cdot}5\sqrt{NP_1} = 1{\cdot}75\sqrt{NP_m}, \text{ nearly.}$$

$$p_t = p_v = p_c = \frac{2}{\sqrt{NP_1}} = \frac{0{\cdot}6}{\sqrt{NP_m}}, \text{ nearly.}$$

* With large ports and dry exhaust steam this figure should be reduced 10 per cent.

† Emery has proposed, as fair values, $r_e = \frac{P+37}{22}$, where P is the *gauge* pressure, and considers the values thus obtained as large for ordinary single cylinder engines, and small for the best compound engines.

In determining the values of the index n in the assumed expression $pv^n =$ constant, for the equation of the expansion line, new difficulties arise. This index is itself variable in each case as expansion progresses, and no two cases give the same mean value. Rankine takes $n = \frac{10}{9}$ for expansion in non-conducting cylinders, assuming that in those engines on which he experimented the conditions were practically such as to give this value. The writer has no doubt that the steam supplied them was practically dry, but Zeuner has shown that the value of n is pretty nearly

$$n = 1 \cdot 035 + \frac{x}{10}$$

where x measures the proportion of steam present or the "dryness fraction." For $x = 1$, $n = 1 \cdot 135$, as tabulated, and for $n = 1 \cdot 111$, as taken by Rankine, we have

$$x = 10\,(1 \cdot 111 - 1 \cdot 035) = 0 \cdot 76,$$

showing that initial condensation must have produced 24 per cent. water, a fact which introduces an error in his estimates of heat supplied to the engine from the boiler.

In none of the values above given is the fact exhibited that the effect of the phenomenon here studied at some length is to cause a rapid fall of the expansion line at the start and a considerable rise at the end, thus causing the line to depart from the curve represented by $pv^n = C$ to an extent that cannot be definitely stated. The value of the index is rendered by this cause, also, not only very variable for different cases, but it is probably usually varied constantly along the expansion line in each case by these new variable conditions. So great are these departures from any laws yet expressed by formulas that we may be justified often in taking advantage of the fact that the curve as often approaches the equilateral hyperbola as any other regular curve.

Where steam-jacketing is so efficient as to prevent condensation during expansion, and where, assuming it possible, superheating can be made so effective as to prevent initial condensation, the transfer of heat from steam to exhaust without transformation into work, in the manner here considered, would be greatly reduced, and perhaps so far as to make the limit $p_v = p_c + p_f$, as in non-condensing engines with low steam.

The compound engine, with receiver, offers peculiar opportunities to secure these conditions by superheating between the two cylinders, as has been done by Corliss, Leavitt and others.

The last column of the large table exhibits this case, assuming $p_v = p_t = p_b + p_f = 7\frac{1}{2}$ lbs. The value of r is increased for a given value of p_t, often to a considerable extent in actual engines, by re-evaporation.

The tabulated values of r_e may be taken as maxima for low pressures and minima for high steam, and in the latter case considerable departures from them in the direction of larger values have often produced but little difference of efficiency.

The following are values of n for various cases commonly found in real work, or taken in theoretical discussions:

VALUES OF n IN pv^n = CONSTANT.

Air, isothermal expansion, . .	1·0
" adiabatic " . .	1·4
" wet and adiabatic, . . .	1·2
Gases generally, isothermal, . .	1·0
" " adiabatic, . .	1·4
" in explosive gas engines, :	1·6
Steam, dry and saturated, . .	1·046
" adiabatic, . . .	1·135
Steam, 0·76 ; water, 0·24, . .	1·111
" superheated, . . .	1·333
Steam and water generally,	$n = 1{\cdot}035 + \dfrac{x}{10}$

The absolute values of the weights of steam used in engines under the conditions above considered cannot be predicted with any greater accuracy than the proper rates of expansion. The expenditure of heat in this method of waste increases in some undetermined ratio with the increase of the rate of expansion, and the writer has usually anticipated a loss at least proportional to the square root of that rate, and would add a percentage equal usually to at least $h_e = 0{\cdot}1\sqrt{r_e}$, and often to $h_e = {\cdot}25\sqrt{r_e}$, to the amount calculated ordinarily by Rankine's methods, and would expect the weight of steam used to reach $W = \dfrac{200}{\sqrt{P_1}}$; $W = \dfrac{24}{\sqrt{P_m}}$; nearly, in general practice and with good engines

Probable Terminal Pressures and Rates of Expansion at Maximum Efficiency.

Initial Pressures. *Absolute.*		Speed of Piston.		Single Cylinders. Non-condensing. Unjacketed.			Jacketed.			Condensing. Unjacketed.			Jacketed.			Compound ($N=2$) Condensing. Sides jacketed.			Heads and sides jacketed.			Heads and sides jacketed with efficient super-heating.		
				$p_t=p_v$		r_e	$p_t=p_v$		r_e	$p_t=p_v=p_e$		r_e	$p_t=p_v=p_e$		r_e	$p_t=p_v=p_e$		r_e	$p_t=p_v=p_e$		r_e	$p_t=p_v=p_b+p_f$		r_e
P_1	P_m	V	m	Lbs. on sq.in.	Kilos on sq.cm.		Lbs. on sq.in.	Kilos on sq.cm.		Lbs. on sq.in.	Kilos on sq.cm.		Lbs. on sq.in.	Kilos on sq.cm.		Lbs. on sq.in.	Kilos on sq.cm.		Lbs. on sq.in.	Kilos on sq.cm.		Lbs. on sq.in.	Kilos on sq.cm.	
40	2·8	400	122	20	1·4	2·0	20	1·4	2·0	16	1·1	2·5	13	·9	3·0	9	·6	4·5	7	·5	6	$7\frac{1}{2}$	·5	5
		625	185	20	1·4	2·0	20	1·4	2·0	13	·9	3·0	13	·9	3·0	9	·6	4·5	7	·5	6	$7\frac{1}{2}$	·5	5
60	4·2	400	122	20	1·4	3·0	20	1·4	3·0	20	1·4	3 0	15	1·1	4·0	11	·8	5·5	8	·6	7·5	$7\frac{1}{2}$	·5	8
		625	185	20	1·4	3·0	20	1·4	3·0	15	1·1	4·0	15	1·1	4·0	11	·8	5·5	8	·6	7·5	$7\frac{1}{2}$	·5	8
80	5·6	400	122	23	1·6	3·5	20	1·4	4·0	23	1·6	3·5	18	1·3	4·5	13	·9	6·5	9	·6	9	$7\frac{1}{2}$	·5	11
		625	185	20	1·4	4·0	20	1·4	4·0	18	1·3	4·5	18	1·3	4 5	13	·9	6·5	9	·6	9	$7\frac{1}{2}$	·5	11
100	7·0	400	122	25	1·8	4·0	20	1·4	5·0	25	1·8	4	20	1·4	5·0	14	1·1	7 0	10	·7	10	$7\frac{1}{2}$	·5	13
		625	185	20	1·4	5·0	20	1·4	5·0	20	1·4	5	20	1·4	5·0	14	1·1	7·0	10	·7	10	$7\frac{1}{2}$	·5	13
120	8·4	400	122	27	1·9	4·5	22	1·5	5·5	27	1·9	4·5	22	1·5	5·5	16	1·1	7·5	11	·8	10·5	$7\frac{1}{2}$	·5	17
		625	185	22	1·5	5·5	22	1·5	5·5	22	1·5	5·5	22	1·8	5·5	16	1·1	7·5	11	·8	10·5	$7\frac{1}{2}$	·5	17
150	10·5	400	122	30	2·1	5·0	25	1·8	6	30	2·1	5·0	25	1·8	6·0	18	1·3	8·5	12·5	·9	12	$7\frac{1}{2}$	·5	20
		625	185	25	1·8	6·0	25	1·8	6	25	1·8	6·0	25	1·1	6·0	18	1·3	8·5	12·5	·9	12	$7\frac{1}{2}$	·5	20
200	14·1	400	122	36	2·6	5·5	29	2·1	7	36	2·5	5·5	29	2·1	7·0	20	1·4	10·0	14	1·0	14	$7\frac{1}{2}$	·5	27
		625	185	29	2·1	7·0	29	2·1	7	29	2·1	7 0	29	2·1	7·0	20	1·4	10·0	14	1 0	14	$7\frac{1}{2}$	·5	27

$p_t=p_v$ is taken as equal to p_e except where $p_v=p_b+p_f$ exceeds p_e, when this greater value is adopted; *e.g.*, in non-condensing engines with low steam, and in highly superheated steam engines.

Deduct 14·7 lbs. per sq. in. = 1 kg. per sq. cm. to obtain gauge pressures. Hyperbolic expansion is assumed.

In the general expression for loss by initial condensation, $h_e = a\sqrt{r_e}$, the writer has used the following values for the coefficient a:

Unjacketed cylinders, . . .	$\begin{cases} 1600 \div Vd \\ 1200 \div V_m\, d_m \end{cases}$
Cylinders with sides jacketed, .	0·10
Cylinders with sides and heads jacketed, .	0·07

in which d and d_m are diameters of cylinders in inches and centimetres and V and V_m are velocities of piston in feet and metres.

Similarly rough statements of probable weight of steam per horse-power and per hour for a wider range of *average* good conditions, as used by the writer, have been (minima)

$$W = \frac{40}{\log.\ P_1};\quad W_m = \frac{7 \cdot 7}{\log.\ P_m}, \text{ for fast or jacketed engines;}$$

$$W = \frac{100}{\log. (P_1 V)};\quad W_m = \frac{20}{\log. (P_m V_m)}, \text{ for unjacketed cylinders.}$$

Where the ratios of expansion have been made those of maximum efficiency, the closest approximation has been attained by taking

$$W = \frac{15}{\log.\ r_e}\,(1 + \cdot 1\sqrt{r_e});\quad W_m = \frac{7 \cdot}{\log.\ r_e}\,(1 - \cdot 1\sqrt{r_e}),$$

as the expenditure will in such cases approximate most nearly to a direct ratio with the net energy obtained per diagram. These values are adopted in the following table. It is evident that the higher the value of r_e, the better the type of engine, and that we are here given a good gauge by which to make comparisons of the efficiency of different kinds of engines.

These values accord moderately well with the observation and experience of the writer where engines of good design have been compared, and may possibly prove useful to others in designing or in drawing up specifications. Like the values of p_v or of r, they can only be taken as probable means, and adopted provisionally, until better and more accurate values have been determined for a wider range of conditions.

Thus, we have the following probable values of weight of steam demanded where the ratio of expansion is correctly adjusted:

Probable Minimum Weights of Steam per Hour per Horse-power.

r_e	W Pounds.	W_m Kilos.	r_e	W Pounds.	W_m Kilos.	r_e	W Pounds.	W_m Kilos.
3	32	15	8	20	9	13	19	8
4	27	12	9	19	9	14	16	7
5	25	11	10	19	9	16	16	7
6	22	11	11	18	9	20	15	7
7	20	9	12	17	8	25	15	7

Taking the probable minimum expenditure of coal per hour and per horse-power at *one-ninth* the weight of steam demanded, we get

$$W' = \frac{1{\cdot}7}{\log.\ r_e}\,(1 + 0{\cdot}1\sqrt{r_e}),\ W'_m = \frac{{\cdot}8}{\log.\ r_e}\,(1 + 0{\cdot}1\sqrt{r_e}).$$

and thus, assuming, as before, the best probable conditions and the ratio of expansion giving a minimum cost of steam, we obtain the following:

Probable Minimum Weights of Coal per Horse-power per Hour.

r_e	W' Pounds.	W'_m Kilos.	r_e	W' Pounds.	W'_m Kilos.	r_e	W' Pounds.	W'_m Kilos.
3	3·5	1·6	8	2·2	1·0	13	1·8	0·9
4	3·0	1·4	9	2·1	1·0	14	1·8	0·8
5	2·8	1·3	10	2·1	1·0	16	1·8	0·8
6	2·3	1·1	11	2·0	0·9	20	1·7	0·8
7	2·2	1·0	12	1·9	0·9	25	1·7	0·8

For cases in which the boiler gives an evaporation of ten pounds of water per pound of coal we may get ten per cent. better figures.*

* A private letter, lying on the table of the writer, giving results corresponding with the case assumed as giving $r_e = 20$, states the coal consumption at 1·5 pounds. This is obtained by one of the oldest and most distinguished engineers in the United States. The boiler has about this maximum efficiency.

So far as the experience of the writer and comparisons made by him with data given by the best experiments have extended, these figures have proven so far accordant that he does not hesitate to use them in estimating probable results. Adding, say, 20 per cent. will give figures on which to base a guarantee in making up contracts, for skillfully designed engines.

The introduction of this element practically completes the theory of the steam engine. Every practicing engineer will look with interest for experimentally derived data and exact expressions that may replace approximate formulas which as here provisionally used are purely empirical and have no scientific value.

How far the high efficiencies here seen to be probably attainable are worth paying for is a commercial question of great importance, but is quite distinct from that here considered.

References. The quantities and the empirical formulas and rules given by the writer as deduced from experience and observation may be compared with the following, which comprise nearly all that he has been able to find bearing upon the subject with any degree of definiteness: D. K. Clarke, "Manual for Mechanical Engineers," pp. 888, 890; "Northcott on the Steam Engine," pp. 157, 158; Isherwood's "Engineering Researches"; "Cotterill on the Steam Engine," chap. xi, especially pp. 294 to 296; R. H. Buel's "Addenda" to "Du Bois' Weisbach," vol. ii, § 512; Rankine's "Papers"; Rankine's "Steam Engine," § 282, 289; "Porter on the Richards Indicator," London, 1874, sect. iii.

New Actinic Phenomenon. M. Phipson prepares a zinc white by precipitating a sulphate of zinc by means of a more or less complete solution of barium sulphide. The precipitate is placed under a hydraulic press and then heated to redness in a furnace, care being taken to prevent too great oxidation. When the experiment is properly conducted and the product is exposed to sunlight the snowy white gives place gradually to a brownish tint, which becomes of a slate color in about twenty minutes. If placed again in the darkness the dark shade is gradually lost, and in the course of five or six hours it becomes as white as snow. The experiment may be repeated with the same specimen as often as may be wished.—*Les Mondes.* C.

THE WOOTTEN LOCOMOTIVE ENGINE.

By J. Snowden Bell.

A review of the recent practice in and present status of American locomotive engineering, will evince the fact that, in their efforts to answer the requirements of increased tractive power and higher speed, constructors have been, as a rule, confined to such enlargement of the capacity of the engine as is resultant upon the increase of its dimensions, without material variation, either in principle or details, from patterns which have become standard for different classes of service. This is particularly the case respecting boilers, the ordinary type, with plain furnace and fire tubes extending from the fire box to the smoke box, having entirely displaced the limited number of special constructions differing materially therefrom, which were devised and made the subject of more or less extended practical tests, at and soon after the general introduction of coal as fuel. The most prominent of these were the Boardman boiler (central flue and hanging fire tubes, 1849), the Dimpfel (water tube, 1850), the Milholland (double series of fire tubes, with intermediate combustion chamber, 1852), and the Phleger (lower and upper water bridges and combustion chamber, 1856), each of which was abandoned, after having been brought into use to an extent sufficient to indicate, in actual service, a lack of practical value. The Norris Locomotive Works, of Philadelphia, built a number of coal-burning engines with the Phleger boiler, and also essayed the introduction of a boiler patented by Wm. G. Norris, in 1857, the latter being analogous to that of Milholland, but their efforts were not crowned with success, and thereafter the ordinary boiler, with an increased proportion of fire surface, became the universal practice.

The necessity of enlarging the grate and fire box area, in boilers for coal-burning engines, met with early recognition, and the late Ross Winans and James Milholland, who were the earliest constructors upon a large scale, first made a compliance with this requisite a special feature in their engines. Each using an overhung fire box, Winans lengthened and Milholland widened it, the increased grate area of the latter being obtained by laterally extending the fire box below the frames and beyond the driving wheels. This construction was not

adaptable to engines for passenger service, having driving wheels of comparatively large diameter on an axle located below or behind the fire box, and the long furnace of Winans, being the only one suited to such conditions, was adopted, under different modifications, and is at the present time the standard for the ordinary anthracite coal-burning passenger engine. The efficiency of the Winans furnace was and is restricted by the limitation placed upon its lateral extension by reason of its position between the wheels, such increase of width as is obtainable by the employment of thin or "slab" frames, or even by the elevation of the furnace above the frames, being comparatively inconsiderable.

Mr. J. E. Wootten, General Manager of the Philadelphia and Reading Railroad, has designed and constructed a locomotive boiler, which is the most recent and most notable departure from the standard American practice. The object sought, and, as evidenced by the performance of the engines, successfully accomplished, was to enable any desired amount of grate area to be employed, in a boiler adaptable to all classes of locomotive engines, and to thereby attain the resultant advantages, not only of perfecting the combustion of anthracite as ordinarily prepared, but also of rendering practicable and economical the consumption of a fuel which it has not been hitherto possible to burn in locomotive furnaces, namely, fine anthracite or "buckwheat" coal (previously a waste product of the Pennsylvania anthracite regions) and lignite, or fossil wood. The distinguishing characteristic of the Wootten locomotive is, in brief, a fire box which is extended laterally over the driving wheels, without materially elevating the waist or body of the boiler. The width of a fire box, upon this plan, may be as great as desired, within the limits fixed by the distance between the tracks, that is to say, as wide as any car which will pass over the road, and the undue elevation of the boiler is avoided by the use of a fire bridge interposed between the furnace and an adjacent combustion chamber within the waist of the boiler, the height of the bridge being equivalent to the depth of the front water leg in an ordinary anthracite coal-burning boiler.

This fire box was designed specially for the utilization, as fuel, of the waste produced in the mining and preparation of anthracite coal. This refuse material amounts to from 20 to 25 per cent. of the output of the mines of Pennsylvania, which for the past year was, in round

numbers, 23,000,000 tons. There is usually mixed with it from 18 to 20 per cent. of slate and other impurities, the elimination of these being effected by the jigging process, which thoroughly separates the coal by agitation in water, the denser slate falling to the bottom, whilst the lighter carbon passes off at the surface. For some time, the unseparated waste, as delivered from the smallest mesh screens, was used in these engines, but the large percentage of contained incombustible matter rendered it necessary to so frequently clean the fire, that it was found more desirable to separate such matter before supplying the fuel to the tender than to perform a corresponding operation in the furnace while in service. It will be obvious that the fine particles thus made available as fuel can only be consumed in a furnace, the draught of which is so gentle as not to lift them from the grate, and it is in this particular that the enlarged grate of the Wootten engine, by the diffusion of the draught over a very large area, serves the purpose of maintaining comparative quietude of the fuel, even under the intense action of the exhaust blast.

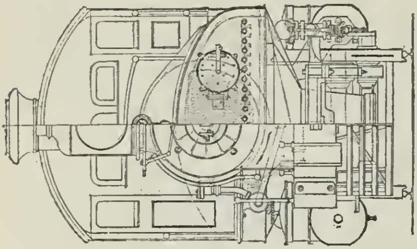


Fig. 2.

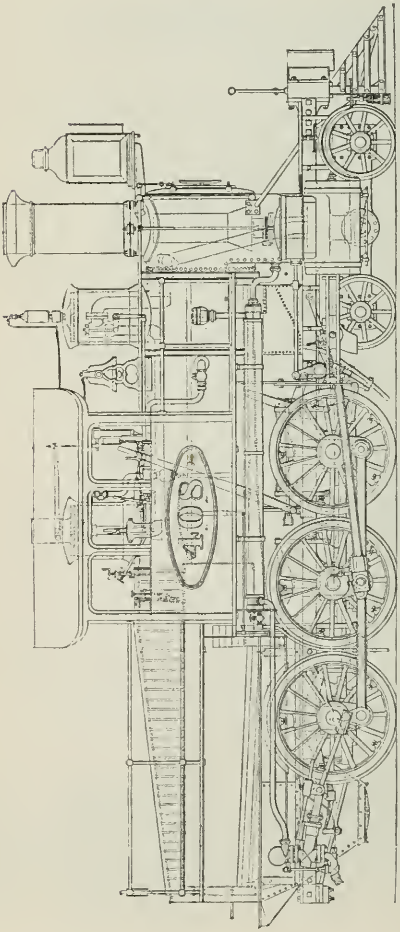


Fig. 1.

The first engine of this construction, No. 408 of the Phil-

adelphia and Reading Railroad, illustrated in Figs. 1 and 2, was built at the Reading shops of that company, and put in service about four years since. It is of the ten-wheeled class, having six coupled driving wheels and a four-wheeled leading truck, is designed for fast freight service, and is of the following general dimensions:

Cylinders, . . .	18 x 24 in.
Diameter of driving wheels, .	54 in.
Diameter of truck wheels, .	30 in.
Wheel base, . .	20 ft. 5 in.
Diameter of boiler at smoke box, .	45½ in.
Diameter of boiler at fire box, .	54 in.
Number of tubes, . .	160
Length of tubes, . .	10 ft. 2 in.
Diameter of tubes (outside), .	2 in.
Length of fire box (inside), .	8 ft. 6 in.
Width of fire box (inside), .	7 ft. 6½ in.
Combustion chamber, .	31 in. long
Grate area, . . .	64 sq. ft.
Heating surface of tubes, .	850 sq. ft.
Heating surface of fire box, .	106 sq. ft.
Heating surface of combustion chamber,	26 sq. ft.
Total heating surface, . .	982 sq. ft.
Diameter of smoke stack, .	20½ in.
Exhaust nozzle, variable from .	4 to 5 in. diam.
Weight on driving wheels, .	67,900 lbs.
Total weight of engine, . .	86,150 lbs.

The performance of this engine was such as to justify the construction of others having furnaces of the same plan, and at the present time there are about 75 in use upon the Philadelphia and Reading and other railroads having access to the anthracite region.

An engine of similar design and dimensions (No. 412) was exhibited at the Paris Exposition of 1878, and was thereafter made the subject of experiments upon Chemin de Fer du Nord, of France, using as fuel anthracite coal and *briquets* composed of pulverized bituminous coal and coal tar. It was also tested upon the Alta Italia Railway, in Italy, by order of the government of that country, using anthracite and the native lignites of various localities. The report of its performance on the latter road, made by the government engineers Fadda, Codazza and Senesi, and dated July 14th, 1879, which has

not heretofore been published, evinces that upon a careful and thorough examination and practical test of its construction and capabilities, these engineers found much in the American machine to approve and nothing to condemn. The developments made in their experiments induced the Northern Pacific Railroad Company to investigate the practicability of burning lignite, which exists in considerable quantities within easy reach of their line, and, accordingly, some ten or fifteen tons of this fuel were forwarded to Philadelphia in December last, for trial in one of the consolidation engines in use on the Reading Railroad. The result was so satisfactory that several engines have been ordered by the Northern Pacific Company, and are now under construction at the Baldwin Locomotive Works. These engines will have 18 x 24-inch cylinders, and a grate area of 81 square feet.

The following summary of the deductions expressed by the engineers in charge of the Alta Italia tests, will indicate the extent and accuracy of their observations, as well as illustrate the capabilities of the furnace with lignite as fuel.

The report states that, with the lignite of Monte Murlo (Tuscany), the average of evaporation of two succeeding trips was 4·20 kilogrammes of water for each kilogramme of fuel. This lignite gave a fine and even fire, without a very long flame, scarcely a perceptible odor of sulphur, no sparks from the stack, and no residue of combustion in the smoke box or tube, "as is ordinarily the case in our locomotives." The latter feature, being the same with all the qualities of fuel employed, is attributed by the engineers to the special form of the furnace, regarding which they note that its very large grate surface allows the air to pass through the grate at a moderate velocity, and without drawing into the tubes with the hot gases, small pieces of fuel or other residue of combustion. Referring to the mode of firing, which is stated to be "regular and demands no excessive fatigue for the fireman," it is noted that "a certain practice only is necessary to throw the fuel to a distance of more than two metres, and to keep it equally distributed over the surface of the grate."

Three experiments with anthracite indicated a ratio of evaporation of 8·36, and this fuel is commended as possessing the very highest evaporative effect. A shallow furnace is held to be indispensable, and it is considered that the Italian engines could not be suited to its use without such modifications as would make them almost identical with the Wootten engine. In this connection, it is noted that

upon one of the trials the fire of *briquets* (artificial fuel) having given out, the steam pressure fell to 45 lbs., but that, with a delay of only ten minutes, the fire was started anew, and a sufficient pressure attained to continue the trip.

Two trips were made using the lignite of Cluduneo (near Treviso). This lignite was almost all dust, but burned very well, with no escape of sparks or traces in the smoke box, giving an average evaporation of 8·66 kilog. of water per kilog. of fuel. The special objection to it is stated to be the large amount of sulphur which it contains. A third trial with the Cluduneo lignite, using the injector instead of the pump and heater, indicated a ratio of evaporation of 8·82.

Cardiff coal dust, used in one trip, burned perfectly, without sparks or deposit in the smoke box, and with a ratio of evaporation of 8·90.

The lignite of San Giovanni (Tuscany) is described as having the external appearance of wood, and is very light and therefore easily carried off with the escaping gases. Nevertheless, it burned quietly, without sparks and with good evaporative power. The lignite of Valdagno burned with a beautiful flame, and gave an excellent result. It is remarked that this, being found in alluvial soil, is mixed with much small gravel, and also contains a certain quantity of pyrites.

As a result of the tests, the engineers express the opinion that with arrangements analogous to the Wootten furnace the Italian fuels can be conveniently consumed in their engines. They say, further: "In the secondary order of fuels, Italy is sufficiently rich, and *it would, therefore, be very desirable that this product should receive careful study, to enable us to emancipate ourselves, in part at least, from dependence upon foreign countries for fuel.*" They commend the excellent qualities of American anthracite, and indicate that the advisability of its adoption depends only upon the determination of the question of its cost and that of the necessary modification of their fire boxes.

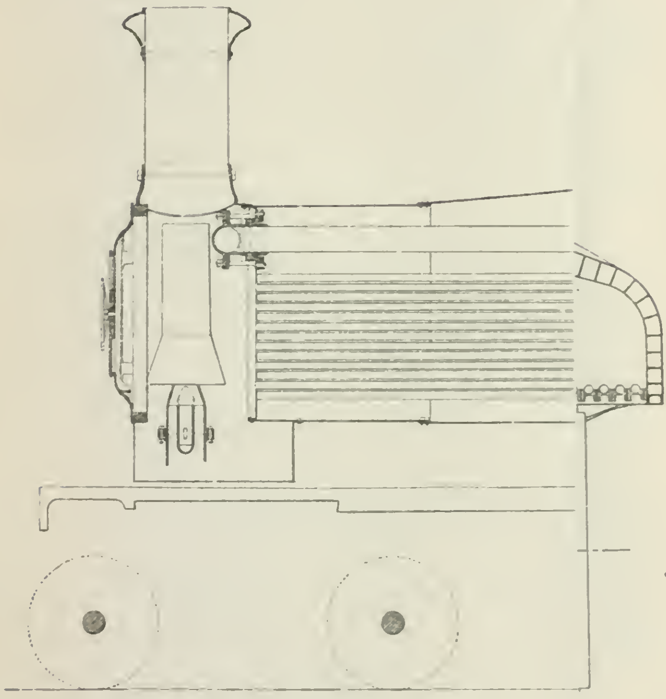
A full description of the engine is given, with notes of approval of sundry points which were novel to the Italian practice, and it is stated that, notwithstanding the form of the engine, its centre of gravity was not unduly high; that it rode with stability, and worked with ease through sharp curves, its truck obviating shocks or oscil-

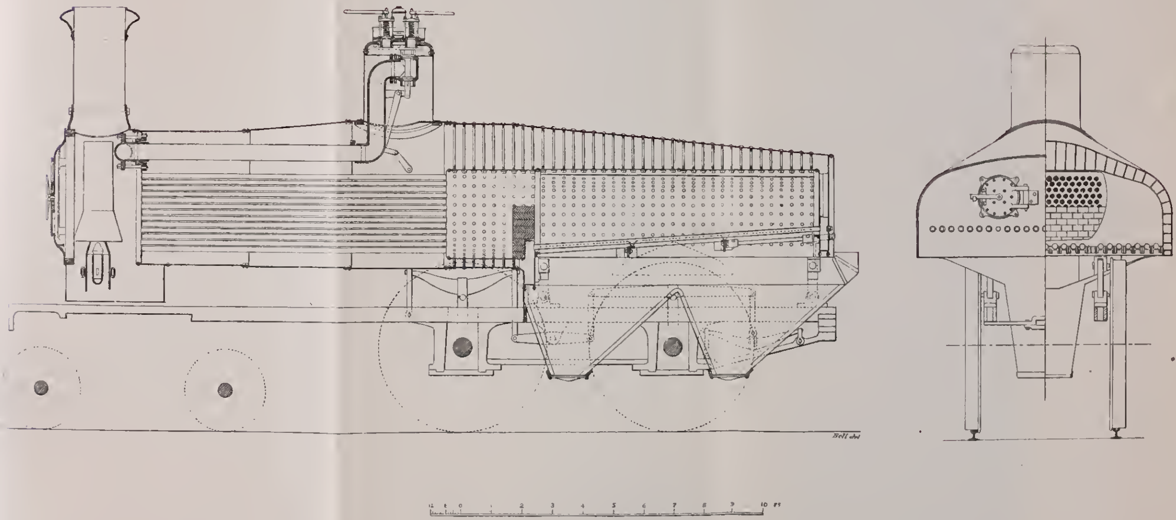
lations, even at velocities of from 40 to 50 kilometres. The report concludes with the statement that, as compared with the Italian engines of the fourth category, the locomotive under consideration is superior in respect of the smaller resistance of its mechanism.

The very free steaming qualities of these engines led to the application of boilers of like design to locomotives intended for passenger service at high velocities, and, although the construction necessarily involves an unusually high centre of gravity in the engine, yet it has been developed by experience, not only that this feature is wholly unobjectionable, but also that it contributes in a marked degree to the steadiness of the engine when running at high speeds upon either curves or tangents. The passenger engines employed in the fast train service have not, as yet, used the waste of the mine as fuel, although in ordinary passenger service its use has been attended with entire success.

An example of a fast express engine, of the largest and most powerful class yet constructed, is illustrated in Figs. 3, 4 and 5, being an elevation and sections of engine No. 411 of the Philadelphia and Reading Railroad, one of a series running fast passenger trains between Philadelphia and Bound Brook. These engines, which were built at the company's Reading shops, exceed in weight, heating surface of fire box, and boiler and cylinder capacity, any passenger engines ever built in the United States. Two of these (Nos. 411 and 506) have cylinders 21 in. x 22 in., driving wheels 5 ft. 8 in. diameter, and weigh 98,200 lbs., of which 64,250 lbs. are upon the driving wheels. Twelve other passenger engines, having boilers upon the same plan, are now in service, in one of which, on the Camden and Atlantic Railroad, bituminous coal is used for fuel with very satisfactory results. In this engine the forward part of the grate has been paved with fire brick, covering about 16 square feet, thus increasing the volume of space available as a combustion chamber relatively to the grate area, and preventing almost entirely the issue of smoke from the stack.

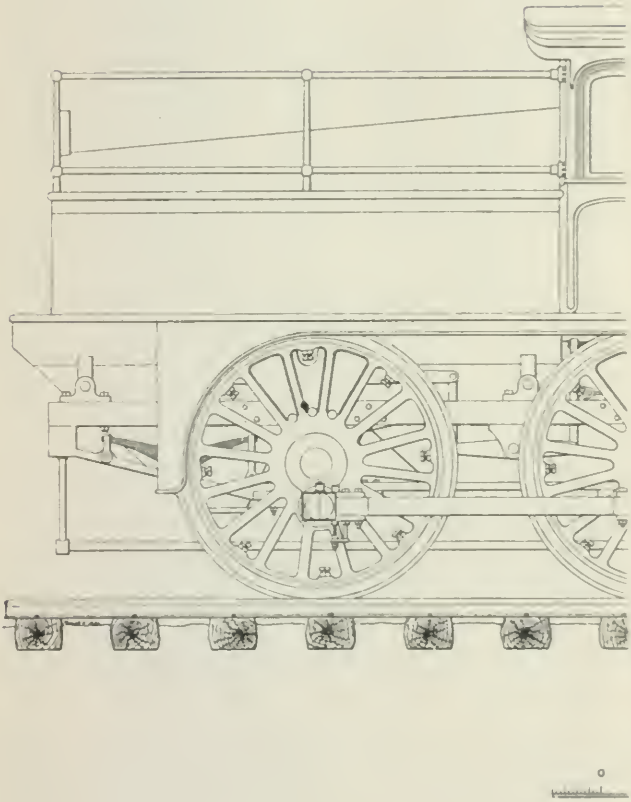
The construction of the boiler, as shown in Figs. 4 and 5, needs but little explanation. The fire box, as in all engines on this plan, is placed entirely above the driving wheels, its exterior width being 8 feet 8 inches, and its length 10 feet 5 inches. The grate area is 76 square feet, and the grate is composed of water tubes and bars, arranged alternately and extending on a slight downward incline from





WOOTTEN'S FAST EXPRESS ENGINE.

Journal of the Franklin Institute, May, 1881.



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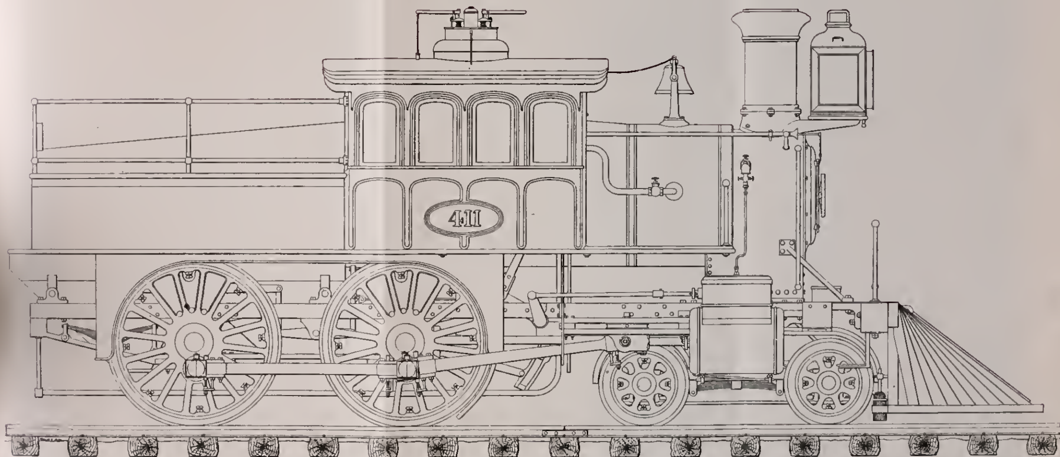


J. Snowden Bell, del.

Scale = 1/32

0 1 2 3 4 5 6 7 8 9 10 11 12 Feet

WOOTTEN'S FAST EXPRESS ENGINE.

A. PARTRIDGE PHOTO LITH. [illegible] ARCH ST. PHILA.

the back of the furnace, in which there are two round firing doors 17 inches in diameter, and placed 38 inches between centres. At the front end of the fire box there is a fire brick bridge, the top of which is 18 inches above the grate. A combustion chamber 27 inches long is formed in the waist of the boiler between the flue sheet and the bridge wall, and is connected by 184 2-inch tubes with the smoke box, which is 54 inches in diameter and 30 inches long from flue head to front. The waist of the boiler is 53 inches in diameter at the smoke box end and 59 inches at its junction with the fire box. There is a single dome 28x28 inches located in advance of the combustion chamber. No crown bars are employed, instead of which the crown sheet is stayed by 279 $\frac{7}{8}$ stay bolts, screwed into the outer and inner casings of the fire box and combustion chamber, their outer ends being riveted over and their inner ends additionally secured by nuts. The use of stay bolts in lieu of crown stays, although by no means a new idea (see T. R. Crampton's French patent, March 24, 1849, and Ross Winan's U. S. patent, May 9, 1854) has latterly been extensively applied, both in this country and in Europe, and so far as the writer's observation and information extend, the system has proven itself to be satisfactory and efficient. A plain open stack, 48 inches high and 21 inches in diameter, the top of which is 14 feet above rail, is used, and there is a register in the smoke box door by which the draught may be diminished by the engineman as required. The exhaust, which has a single nozzle, is variable, by means of an internal cone in the exhaust pipe, which can be raised and lowered, so as to vary the exit opening between limits corresponding to the areas of $3\frac{7}{8}$ inches minimum and $5\frac{3}{4}$ inches maximum diameters respectively.

The cylinders are secured to each other and to the frames on the "half saddle" plan, now universally adopted in the United States, and, owing to the peculiar form of the furnace, a novel spring arrangement and suspension of the rear end of the boiler has been adopted. The smoke box is bolted firmly to flanges on the saddle portions of the cylinder castings, and such longitudinal movement of the boiler as may be induced by its expansion and contraction under the influence of changes of temperature is admitted by means of links, by which the furnace is connected, near its front and rear ends, to pins resting in sockets bolted to the frames. The springs of the front or main driving axle are arranged in the usual manner, and their rear hangers are connected to equalizing bars located below the frames.

The springs of the rear axle are placed behind the pedestals and below the frames, their hangers being connected to the lower frame braces; and the weight transmitted to the rear driving boxes is borne by long equalizing bars, resting on pins on the tops of the boxes, and having hangers at their ends, connected respectively to the centres of the rear springs and to the back ends of the main equalizing bars. The arrangement is simple in its structure and conveniently located, and the remarkably easy riding of these engines, at speeds of 60 miles and over per hour, indicates the action of the springs to be properly and efficiently exerted.

The boiler is fed by two No. 9 Sellers injectors, the duty of each of which is rated by the makers at 45 gallons per minute, and the capacity of the tender is 4500 gallons of water and 10,000 pounds of coal, which admits of making the run from Philadelphia to Jersey City, 89·4 miles, without the necessity of stoppage for water, whilst sufficient fuel is taken for the round trip. The driving and truck wheels have steel tires, and the running gear is of the standard American construction and does not therefore require special notice. Facili- are embodied for oiling bearings from the cab, and also for turning water upon bearings from the tank.

The general dimensions of engine 411 are as follows:

Cylinders, . .	21 ins. diam., 22 ins. stroke.
Driving wheels, . .	diam. 5 feet 8 ins.
Centre to centre of driving axles, .	7 feet
Truck wheels, . .	diam. 33 ins.
Boiler, . . .	diam. 58 to 52 ins.
Number of tubes, . . .	184
Diam. " . . .	2 ins.
Length " . .	10 feet $2\frac{1}{2}$ ins.
Length of furnace, . .	9 feet 6 ins.
Width " . . .	8 feet.
Grate area, . . .	76 sq. feet.
Heating surface of flues, . .	982 "
" " furnace, . .	135 "
Total heating surface, . .	1117 "
Smoke stack, diam., . .	21 ins.
" " height, . . .	48 ins.
Exhaust, single nozzle (variable),	$3\frac{3}{4}$ to $5\frac{3}{4}$ ins. diam.
Total weight of engine, .	98,200 pounds.

Weight on drivers, . 64,250 "

Wheel base, . . . 21 feet 1 in.

The service in which the passenger engines of this class are employed involves the necessity of developing extremely high speed, and of hauling heavy as well as light trains; the following details of their performance will indicate the nature and extent of their duty and capabilities.

In June, 1880, engine 506 hauled 15 passenger cars, carrying nearly 900 passengers, from Philadelphia to Bound Brook, the ruling gradient being 59 feet rise per mile, at an average speed of 42 miles per hour, the aggregate weight of train and passengers, exclusive of engine and tender, being in excess of 360 tons. The average consumption of fuel per mile was 62 pounds, or at the rate of 34½ pounds per hour for each square foot of grate.

In July, 1880, engine 411 hauled 10 fully loaded passenger cars from Philadelphia to Bound Brook (59·2 miles) in one hour and nineteen minutes, making the usual slowing over two miles of bridging, the average consumption of fuel being 54 pounds per mile or 32 pounds per hour per square foot of grate.

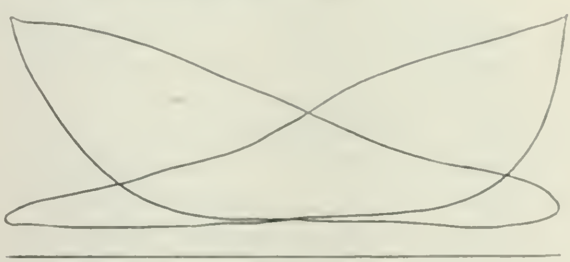


Fig. 6.

The indicator diagram, Fig. 6, was taken on engine 411 on the 20th of December, 1880. The day was one of the coldest of the season, the thermometer marking 6 degrees below zero (Fahr.); the train consisted of four well filled passenger cars. Notwithstanding the unfavorable influence of a brisk north wind, a speed of 72 miles per hour was attained upon a level while cutting off steam at 8¾ inches. Several miles were run continuously in less than 50 seconds. The diagram, the vertical scale of which is 80 pounds to the inch, was taken at a speed of 72 miles per hour (or 360 revolutions per minute) with a boiler pressure of 105 pounds and cutting off at 8¾ inches.

The diagram, Fig. 7, was taken on the same engine, May 4th, 1880. The train consisted of seven passenger cars, and the speed at the time of taking the diagram was at the rate of 64 miles per hour; boiler pressure 123 pounds, and point of cut off $6\frac{1}{2}$ inches.

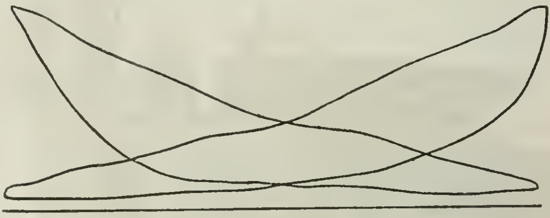


Fig. 7.

The engines above referred to, as well as others of similar design on the Bound Brook Division of the P. & R. R.R., are daily making four trips between Bound Brook and Philadelphia, aggregating 240 miles per day, yet so extremely gentle is the draught of the furnace that it is only occasionally that it becomes necessary to thoroughly clean the fire before the termination of the day's work. The regular schedule time on fast line between Wayne Junction and Bound Brook (54·9 miles) is 64 minutes, including one stop and slowing down three times. This involves an average speed of 56 miles per hour for nearly 55 continuous miles. The performances here noted are not in any manner due to essential differences in the working parts from the ordinary standards of American construction, but simply to the ability of the boiler to generate steam of sufficient volume and pressure to comply with the exacting requirements of the service referred to.

In the run from Philadelphia to Bound Brook there are ten miles of the line having ascending gradients varying from 15 to 30 feet per mile, eleven miles having ascents varying from 30 to 40 feet per mile, and one and a-quarter miles having an ascent of 59 feet per mile; twelve miles of the line are practically level, and there are twenty-five miles of descending gradients varying from 6 to 37 feet per mile. The four miles from Ninth and Green streets to Wayne Junction, being within city limits, are run at such restricted speed as to require eleven minutes to traverse the distance.

The engine shown in Fig. 8, which is reduced from a full page engraving in the *Railroad Gazette*, is one of thirty of the consolida-

tion type, having the Wootten boiler, which were built by the Baldwin Locomotive Works, and are now in coal train service on the Philadelphia and Reading Railroad. These boilers are similar in construction to that of the passenger engine before described, differing therefrom

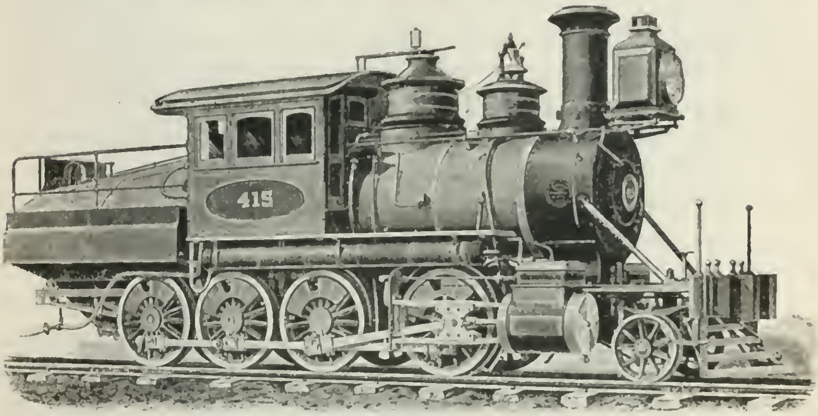


Fig. 8.

only in dimensions and in being fitted with feed-water heaters. The principal dimensions of the engines are as follows:

Cylinders,	20 ins. diam.x24 ins. stroke.
Driving wheels,	diam. 50 ins.
Truck, "	" 30 ins.
Driving wheel base,	14 ft. 9 ins.
Total wheel base,	22 ft. 10 ins.
Fire box, length inside,	9 ft. 6 ins.
" width "	8 ft. 0 ins.
Grate surface,	76 sq. ft.
Boiler, diam.,	59 to 53 ins.
Number of tubes,	197
Diameter "	2 ins.
Length "	11 ft. 6½ ins.
Heating surface of tubes,	1190 sq. ft.
" " fire box,	167 sq. ft.
Total heating surface,	1357 sq. ft.
Driving wheel journals,	7x8 ins.
Truck " "	5x8 ins.
Steam ports,	16x1¼ ins.
Exhaust "	16x2½ ins.

Total weight of engine, . 103,000 pounds.
Weight on drivers, . . 90,000 pounds.
Two No. 8 Sellers injectors for supplying boiler.

In ordinary service the duty of the consolidation engines is to haul trains of 140 loaded 4-wheel coal cars (or their equivalent in 8-wheel cars), averaging 1120 tons gross weight of cars and lading, from Palo Alto to Port Richmond, a distance of 95 miles, at an average speed of ten miles per hour, over level and descending grades. Their consumption of fine anthracite coal in this service averages 11,500 pounds for the trip. On the return trip of 95 miles, in which distance an elevation of 625 feet has to be overcome, the load of these engines is 160 empty cars, and the weight of fine anthracite coal consumed is about 12,000 pounds.

In the month of September last a comparative test was made with one of these engines and an engine of the same type, but having a furnace of the ordinary construction, both engines burning bituminous coal and hauling coal trains of as nearly similar weight as attainable. The relative economy of the respective boilers will appear by the following statement; the distance run in each case was 110 miles daily:

	Consolidation Engine with ordinary boiler.		Consolidation Engine with Wootten boiler.	
Miles run per trip, . .	$110\frac{1}{2}$	$110\frac{1}{2}$	$110\frac{1}{2}$	$110\frac{1}{2}$
Coal used per trip, pounds	10,756	10,804	8,530	7,577
Coal used per mile, pounds	97·3	97·7	77·2	68·5
Water used per trip, gallons	8,871	8,341	9,198	8,305
Water evaporated per pound of coal, pounds, . .	6·87	6·35	8·74	9·13

Showing reduced consumption of fuel equal to 25 per cent. and an increased evaporative efficiency of 35 per cent. in favor of the Wootten boiler.

The relative quantities of prepared coal of ordinary merchantable sizes and of the mine waste, consumed respectively by locomotives of the usual type and by those having the Wootten boiler, are shown by the following data of their performance, viz.:

In 27 days freight train service.

10-wheel engine, ordinary furnace, burned	110 tons broken coal.
" Wootten " "	113 " waste "

In 6 days coal train service.

10-wheel engine, ordinary furnace, . .	30 tons steamboat coal.
Consolidation engine, Wootten furnace, .	33 " waste "

In 42 days coal train service (on gradients 90 ft. per mile).

10-wheel engine, ordinary furnace, . .	313 tons lump coal.
Consolidation engine, Wootten furnace, .	314 " waste "

The economic value of the boiler will be apparent from the fact that the difference between the cost of the fuel used in this furnace and that of ordinary marketable coal, such as is used generally in locomotive furnaces, is two dollars and ten cents per ton; it follows that if there is a daily consumption of 4½ tons of combustible per engine, each locomotive fitted with this boiler will effect a saving of $9.45 per day, a sum quite sufficient for its repair and perpetuation. The exemption from emission of sparks and cinders from the stack, which is noted in the report of the Alta Italia tests before referred to, and which is an obvious consequence of the lighter exhaust under which the enlarged fire box can be operated, while a subordinate feature, is one of material importance. A long series of failures marks the record in the matter of spark arresters, and in this department, as with many other efforts to remedy the evils of defective construction or abnormal operation, the old adage that "prevention is better than cure" is of peculiar application.

An article on "American High Speed Locomotives," published in the London *Engineer* of February 11th, 1881, referring more particularly to the Wootten engine, contains a number of statements which American engineers cannot fail to find both novel and amusing and may be here noticed in comparison with the Italian report, as illustrating the widely different conclusions arrived at, on the one hand, by an intelligent examination and carefully conducted practical test, and, on the other, by a cursory review of drawings and descriptions, evidently made with an insufficient understanding of the subject matter, and seemingly under the limitations of some little prejudice. Characterizing the Wootten engine as "abnormal in design," the article proposes to consider "why it is"; no explanation is, however, given, either of the element or elements of the design that are alleged to be "abnormal," or of the reasons upon which the opinion is based. The statements which are made, that two classes of the engine are built, in one of which "there is only one pair of drivers," and that "perforce Mr. Wootten was compelled to use single drivers," seem surprising, in view of the statement of Mr. Wootten to the writer that he never contemplated the application of a furnace upon his plan in

connection with single drivers, and is wholly unaware of such an engine having ever been made. The assertion that "the road may be regarded as level, the inclines compensating each other," embodies an error of fact, as may be seen from the statement of the profile of the road hereinbefore given, and the assumption as to "compensating" inclines being equivalent to a level, will scarcely be concurred in, either by those who construct railroads or those who operate them.

After a series of calculations, based upon data which the author of the *Engineer* article admits he has no definite or accurate knowledge of, and which appear to be intended to prove that the actual performance of the engine was not equal to that which had been assigned to it, the proposed disclosure of the "abnormal" design is abruptly abandoned in terms by the statement that "We do not propose here to say anything concerning the Wootten locomotive, either as a vehicle or an engine."

The article then proceeds to inform American engineers that until they "give up anthracite coal they will never attain perfection in fast locomotives," and that "English engineers will tell them, to a man, that unless anthracite coal can be burned in a furnace of less dimensions than that used by Mr. Wootten it had better not be burned at all in a fast locomotive." Leaving us wholly in ignorance of the grounds on which these remarkable dicta are based, the article concludes with the oratorical statement that "It is generally conceded that the boilers are the worst things about American locomotives, but without good boilers there can be no really express work done on a railway." Conceding the latter portion of the statement as undeniable, it is mildly suggested that there may be room for a difference of opinion as to the former, and inasmuch as the only "abnormal" feature of the Wootten boiler which appears, even by implication, is the fact that, in accordance with its design, it uses anthracite as fuel, it is probable that American engineers, many of whom are troubled with a large supply of this somewhat useful article, will be sufficiently indulgent to pass the peculiarity objected to without very severe condemnation, and doubtless, some will be found who will even agree with the engineers who conducted the Alta Italia tests in the opinion that the excellent qualities of anthracite coal make it a very desirable fuel for locomotive engines.

THE EFFICIENCY OF THE ENGINES OF THE STEAMER "ANTHRACITE."

By C. R. ROELKER, Passed Assistant Engineer, U. S. Navy.

We possess so few complete and reliable records of trials with steam machinery which can be used for an investigation of the various influences affecting the efficiency of steam in an engine that all additional experimental data relating to this subject should be thoroughly analyzed. The data furnished by the carefully conducted trial of the machinery of the steamer *Anthracite*, made in August, 1880, at the New York navy-yard, possess special interest because the means by which modern steam engineering seeks increased economy, viz.: high initial steam pressure and a high degree of expansion—were carried in this case to a limit which had not been heretofore approached. A full account of this trial and of the results obtained has been given in a paper "On the Experiments with the Perkins Machinery of the Steam Yacht *Anthracite*," published in No. 1, 2 and 3 of the current volume of the FRANKLIN INSTITUTE JOURNAL. In this paper the discussion of the action and efficiency of the steam in the engine is based on a comparison of the weights of water evaporated in the boiler, found by actual measurement, and of steam condensed in the different cylinders, calculated from indicator diagrams. But the weight of steam expended is not an exact measure of the efficiency of an engine, and, besides, the method by which the work done by the steam and the weight of steam condensed in the cylinders is calculated is, to say the least, unusual. These considerations have induced the writer to present the following discussion of the efficiency of the *Anthracite's* engines in this trial, as measured by the number of units of heat expended in the engines, so that the results of this trial may be directly comparable with those obtained in other steam engine trials.

The calculations in this paper are based on the data recorded in the published official report of this trial. It must, however, be observed that the *total horses-power* developed in each cylinder are computed in the usual manner for the mean absolute pressure on the *whole* area of the piston. Further, the *indicated horses-power* developed in the first

or small cylinder is not calculated from the mean ordinate of the indicator diagrams taken from the top of that cylinder, but for the difference between the mean absolute steam pressure in that cylinder during the down stroke of the piston and the mean back pressure acting on the under side of the piston during that stroke; in this manner the actual gross effective power developed during the down stroke of the piston in that cylinder is found. In like manner, the *indicated horses-power* developed in the second or intermediate cylinder are calculated for the difference between the mean absolute pressure on the piston of that cylinder during the upstroke and the mean back pressures acting on the upper side of the piston of the small cylinder and on the annular surface of the piston communicating with the receiver. Thus we get the following quantities:

Total horses-power developed in first cylinder, .	28·8902
Total horses-power developed in second cylinder, .	31·8339
Total horses-power developed in third cylinder, .	52·9109
Indicated horses-power developed in first cylinder, .	23·2280
Indicated horses-power developed in second cylinder, .	5·1000
Indicated horses-power developed in third cylinder, .	39·4483

For other data used in the following calculations the reader is referred to the published official report. In order to make the results obtained in the present investigation comparable with those given in the above mentioned paper relating to this trial, the same constants have been used in these calculations as in the former paper: for this reason the mechanical equivalent of one Fahrenheit unit of heat is taken at 789·25 foot pounds.

1. Units of heat generated in the boiler per hour, .	1,642,983

First, or Small, Cylinder.

2. Units of heat equivalent to indicated horses-power developed in first cylinder, per hour, . .	58,271
3. Units of heat equivalent to total horses-power developed in first cylinder, per hour, . .	72,477
4. Units of heat in the steam discharged from first cylinder at the end of the stroke of its piston, less the units of heat in an equal weight of feed-water, per hour, ...	620,711

5. Units of heat in the condensed water present in the first cylinder at the end of the stroke of its piston, less the units of heat in an equal weight of feed water, per hour, ...	174,035
6. Units of heat expended in radiation, conduction, etc., and in re-evaporation at the end of the stroke, in the first cylinder, per hour, . . .	775,760

Second, or Intermediate, Cylinder.

7. Units of heat equivalent to indicated horses-power developed in second cylinder, per hour, . .	12,794
8. Units of heat equivalent to total horses-power developed in second cylinder, per hour, . .	79,861
9. Units of heat in the steam discharged from second cylinder at the end of the stroke of its piston, less the units of heat in an equal weight of feed-water, per hour, ...	809,855
10. Units of heat in the condensed water present in the second cylinder at the end of the stroke, less the units of heat in an equal weight of feed-water, per hour,	87,738
11. Units of heat expended in radiation, conduction, etc., in first and second cylinders, and in re-evaporation at end of stroke of piston in second cylinder, per hour,	607,258

Third, or Large, Cylinder.

12. Units of heat equivalent to indicated horses-power developed in third cylinder, per hour, . .	98,964
13. Units of heat equivalent to total horses-power developed in the third cylinder, per hour, . .	132,736
14. Units of heat in the steam discharged from the third cylinder at the end of the stroke of its piston, less the units of heat in an equal weight of feed-water, per hour, ...	1,144,020
15. Units of heat in the condensed water present in the third cylinder at the end of the stroke, less the units of heat in an equal weight of feed-water, per hour,	21,342
16. Units of heat expended in radiation, conduction, etc., in first, second and third cylinders, and in re-evaporation at end of stroke of third cylinder, per hour, .	273,820

In computing the units of heat transmitted to the feed-water in the boiler, per hour (1), it has been assumed that the steam was dry and saturated in the boiler. The temperature and dryness of the steam were not ascertained during the trial by thermometric and calorimetric tests; but the board which conducted this trial has based the calculations given in the report on the same assumption. The total heat of the feed-water is taken at 120·63 Fahrenheit units, and the total heat of the boiler steam is taken at 1242·03 units, above the Fahrenheit zero.

Since a careful examination of the pistons and valves under pressure revealed no leakage of steam it is assumed that, at the end of each stroke, the whole weight of water vaporized in the boiler during the corresponding time was present in each cylinder. The weight of steam discharged from the cylinder at the end of the stroke is calculated by multiplying the space displaced by the piston per stroke plus the space in the clearance and steam passage, by the density of steam corresponding to the final pressure in the cylinder, and subtracting the weight of steam filling the clearance and steam passage at the moment the steam valve begins to open. The units of heat present in this weight of steam above the heat of the feed-water (4, 9, 14) are found by multiplying this weight of steam by the total heat of steam of corresponding pressure above the heat of the feed-water, less the quantity of heat expended in overcoming external resistance in vaporizing the feed-water under the pressure obtaining at the end of the stroke.

The difference between the weight of steam found to be present in the cylinder at the end of the stroke and the weight of water vaporized in the boiler is called the weight of condensed water present in the cylinder at the end of the stroke for the unit of time, and it includes the weight of steam admitted to the jacket-coils during the same time. It is to be regretted that the weight and temperature of the water discharged from the jacket-coils were not measured in the experiment. In computing the units of heat present in the condensed water in each cylinder (5, 10, 15) it is assumed that all this water had the temperature of the saturated steam at the end of the stroke of the piston in the respective cylinder. Strictly speaking, this assumption is not correct, since the pressures and temperatures in the jacket-coils of the different cylinders differed probably from those in the cylinders at the end of the stroke; but the resulting error is necessarily small.

This error makes the quantities of heat expended in radiation, re-evaporation, etc., as given above, a little too large, and the quantities of heat contained in the water present in the cylinder and in the jacket, at the end of the stroke, a little too small.

The quantities of heat given as expended in radiation, conduction, etc. (6, 11, 16), are simply the quantities left after deducting from the total heat transmitted to the feed-water in the boiler the sum of the quantities of heat given as being present in the steam and water in the cylinders and of the heat transmuted into power. The neglect to measure the weight and temperature of the water discharged from the jackets makes a further analysis of these quantities impossible. If the steam generated in the boiler should have been either moist or superheated, instead of dry saturated as assumed, the quantities given in 6, 11 and 16 would, of course, be correspondingly less or greater. A relatively small portion of this heat is actually lost by radiation, conduction and vibrations; the larger portion is expended in warming the walls of the cylinders cooled during the previous return-stroke by the vaporization of water deposited upon them; and, in consequence of a like process, this heat re-appears and is available for work in the second and third cylinders, but is discharged from the third cylinder into the condenser. The absorption of heat due to this cause in the larger cylinders is greatly less than in the smaller cylinder, because in the larger cylinders the range of temperature of the steam and the ratio of the superficial area of the walls to the volume of the cylinder is greatly less, and the temperature of the steam entering the jacket relatively to the mean temperature of the steam in the cylinder is greater than in the smaller cylinders.

Of the heat transmuted into external work only that portion which is equivalent to the indicated horses-power developed in those cylinders has permanently disappeared, the remainder is expended in overcoming the resistance of the steam acting on the opposite side of the piston and causes an equal amount of energy to re-appear in this steam, which becomes available in the second and third cylinders respectively.

After the steam has completed its expansion in the third cylinder when the piston has arrived at the end of its stroke, 78·54 per cent. of the total weight of water vaporized in the boiler is present in this cylinder in the form of steam, the remaining 21·46 per cent. being condensed in this cylinder and in the jacket. Of the total heat transmitted to the water in the boiler 70·93 per cent. are found to be pre-

sent in the steam and water filling the third cylinder and the jacket-coil; 12·40 per cent. have been transmuted into external work represented by the indicated horses-power developed in the first and second cylinders and the total horses-power developed in the third cylinder; and 16·67 per cent. have been lost through radiation, conduction, etc., in the passage of the steam from the boiler to the end of the stroke in the third cylinder, and through the abstraction of heat from the walls of this cylinder during the exhaust-stroke by the re-evaporation of the water deposited upon them. Besides this latter quantity (the amount of which we cannot determine from the data of the experiment) there is discharged into the condenser the whole of the heat given as being present in the steam and water filling the cylinder at the end of the stroke of the piston plus the heat equivalent of the difference between the total and indicated horses-power developed in the third cylinder (if we neglect the small amount of work done during compression), or [70·93 + 2·06 =] 72·99 per cent. of the total heat transmitted to the water in the boiler.

The quantity of heat transmuted into mechanical work represented by the indicated horses-power developed by the engines is 10·34 per cent. of the total heat of evaporation, and this represents the *actual efficiency* of the engines.

The *relative efficiency* of the engines is found by comparing their actual efficiency with the greatest efficiency theoretically attainable in a steam engine working with saturated steam of the same boiler pressure and with the same temperature of feed-water and transforming all the heat expended (except the quantity necessarily discharged into the condenser) into mechanical work. Such a comparison will enable us to determine what increase of efficiency can possibly be obtained by changes in the practical working of the engines.

In a "perfect reversible heat-engine," working between the limits of temperature given by the boiler pressure and the temperature of the feed-water in the experiment under consideration, the efficiency would be represented by $\left[\frac{426{\cdot}3 - 461{\cdot}2}{426{\cdot}3 + 120{\cdot}5} =\right]$ 34·46 per cent. of the total heat generated.

Since, however, one condition of the property of reversibility in a perfect engine—viz.: that the temperature of the working fluid be raised by compression to the initial temperature, before it is delivered into the boiler—is not realized in our present type of the steam engine, its

efficiency is correspondingly diminished. The loss of efficiency due to this cause, computed by the formula given by Cotterill, is, in the present case, 4·04 per cent. of the total heat of evaporation. Hence, in a perfect steam engine, working with the same boiler pressure and feed-water temperature as in the experiment under consideration [34·46 — 4·04 =] 30·42 per cent. of the total heat of evaporation would be transformed into indicated horses-power and 69·58 per cent. would be discharged into the condenser; and the *relative efficiency* of the *Anthracite's* engines was $\left(\frac{10{\cdot}34 \times 100}{30{\cdot}42} = \right)$33·99 per cent.

The loss of efficiency, represented by [30·42 — 10·34] = 20·08 per cent., is due to (1) the excess of back pressure in the third cylinder above the pressure corresponding to the temperature of the feed-water; (2) the incomplete expansion of the steam, which in the perfect engine is continued till the final pressure is equal to the pressure corresponding to the temperature of the feed-water; (3) the loss of heat by radiation, conduction, re-evaporation of water in the cylinder, etc. By calculation we find the following values for these several losses, expressed in per cent. of the total heat transmitted to the feed-water in the boiler, viz.:

(1) Loss due to excess of back pressure, .	1·30 per cent.
(2) Loss due to incomplete expansion, .	3·55 per cent.
(3) Loss due to radiation, re-evaporation, etc., .	16·67 per cent.
Total loss, . . .	21·52 per cent.

The difference [21·52 — 20·08 =] 1·44 per cent. is due to inaccuracies in the computations and in the constants used, but mainly in the observations made. We may safely conclude that the loss due to radiation, re-evaporation, etc., as given is somewhat too great, either for the reason given above, or because the total quantity of heat transmitted to the feed-water in the boiler was somewhat less than we have assumed it to be, on account of a small percentage of moisture contained in the steam.

The excess of back pressure in the experiment amounted to 2·675 pounds. It is practically impossible to prevent this excess entirely. Assuming that this excess can be reduced to 1 pound the resulting gain in efficiency will be 0·814 per cent.

The unavoidable excess of back pressure will reduce still further the small gain theoretically possible from completing the expansion of the steam. It appears likely that, in the present case, any gain in

efficiency resulting from the use of a higher degree of expansion would be accompanied by equal or greater losses of heat by conduction and re-evaporation, by friction, etc.; consequently, we must conclude that the loss of efficiency due to incomplete expansion cannot be diminished.

The loss of heat due to external radiation and conduction has been determined directly in several experiments with different engines, and has beeen found to vary between 1·5 and 2·5 per cent. Assuming a mean value of 2 per cent. for this loss in the present case, and that the quantity of heat actually lost by external radiation and re-evaporation was 1·44 per cent. less than the quantity given above, we have a loss of [16·67 — 1·44 — 2·00] = 13·23 per cent. due to the transmission of heat from the walls of the third cylinder during the exhaust stroke. This loss may be greatly diminished by efficient steam-jacketing and superheating; it is, however, doubtful whether this loss can be reduced to less than 4·5 per cent. under favorable conditions of practice. Assuming this amount of loss to be unavoidable, we find that the greatest gain in efficiency which may be obtained under more favorable conditions of working in the *Anthracite's* engines is equal to [13·23 — 4·5 + 0·814] = 9·544 per cent. of the total heat transmitted to the feed-water, or 92·3 per cent. of the actual efficiency of the engines.

The large amount of condensation in the first cylinder indicates that the steam may be superheated in the boiler to a considerable degree without producing injurious effects in the engines. To reduce materially the "exhaust waste," or loss from re-evaporation, in the third cylinder, it will probably be necessary to dry or superheat the steam in the receiver before it enters the third cylinder. Such a method of treating the steam in a compound engine was recommended many years ago by Hirn, and has been practically tried in several instances in France.

No account has been taken in the preceding investigation of the heat produced by the friction of pistons and valves, as no safe estimate can be made of its quantity, which is but small at all events.

The very unequal development of the indicated horses-power in the different cylinders was likely prejudicial to the economical performance of the engines. It is desirable that in future trials of these engines attention be given to a more equal distribution of the indicated power among the different cylinders.

AN ACCOUNT OF THE EXPERIMENTS MADE IN MULHOUSE, GERMANY, BY A COMMITTEE OF THE INDUSTRIAL SOCIETY OF THAT CITY, ON A CORLISS STEAM ENGINE, TO DETERMINE ITS ECONOMIC PERFORMANCE WITH AND WITHOUT STEAM-JACKETING.

By Chief Engineer ISHERWOOD, U. S. Navy.

The experiments described in this paper were made by a committee of the Industrial Society of Mulhouse, in Alsace, Germany, on a Corliss engine that had been about six months in use driving the looms of the factory of Messrs. Schlumberger & Co., in that city. The committee consisted of twelve persons, one of whom was a member of the firm which constructed the engine, and their report will be found on pages 910 to 931 of the Bulletin of the Society for 1878.

The cylinder of the engine experimented with was not only steam-jacketed on the sides and both ends, but its piston was made hollow and filled with steam of the boiler pressure, like the jackets of the cylinder, in order that the application of steam-jacketing might receive its utmost extension. The boiler furnished saturated steam. Neither the idea nor the execution of this method of converting the piston into a steam jacket was new, nor were the experiments made to ascertain its effect upon the economic performance of the engine a novelty, as precisely the same had been done in the city of New York by Henry Waterman, in 1864, an account of whose engine, and a complete detail of the experiments made with it, were given in a memoir by the writer to the Bureau of Steam Engineering in the Navy Department of the United States, and published as an appendix to the report for 1875 of the chief of that bureau to the Secretary of the Navy.

The experiments made by the Committee of the Industrial Society of Mulhouse are much inferior to those made by Waterman, in number, importance, variety, length and completeness, but they have the advantage of being made on a greatly larger engine, and one whose valve gear is considered by many to be peculiarly adapted for the economic use of steam.

Instead of furnishing a translation of the report of the committee, which would be of but little use to the American engineer, the writer has taken their observations, recalculating the quantities and correcting them where necessary, following his own arrangement entirely and giving his own deductions, his purpose being to present the data and results of the experiments in the most compact form and yet include every fact ascertained.

Engine.

There were two Corliss engines, each of one cylinder, connected to the same main shaft, but only one engine was experimented with; the other, however, was kept in operation at the same time for the purpose of maintaining a uniform number of revolutions of the main shaft per minute, which was necessary for the work of the factory, during the variable loads employed with the engine under course of experiment.

The cylinder was horizontal, and connected directly to the main shaft, which carried a toothed fly wheel gearing into a pinion, the pitch line diameters of the two comparing as 192 and 65. The diameter of the pitch circle of the fly wheel was $16\frac{2}{3}$ feet. The fly wheel shaft was $8\frac{1}{3}$ feet long and $11\frac{2}{3}$ inches diameter. The shaft of the pinion wheel was $6\frac{2}{3}$ inches diameter and $35\frac{1}{2}$ feet long up to its coupling with the shafting of the factory. The cylinder had the following dimensions:

Diameter of cylinder, . . .	24 inches.
Diameter of piston-rod (both sides of piston),	$3\frac{5}{8}$ inches.
Stroke of piston, ...	48 inches.
Net area of piston, . . .	442·0698 sq. ins.
Space displacement of piston per stroke, .	12·279717 cu. ft.
Clearance, ...	0·3937 inch.
Space in clearance at one end of cylinder, .	0·100193 cu. ft.
Space in steam passages at one end of cylinder,	0·202470 cu. ft.
Total space in clearance and steam passages at one end of cylinder, . . .	0·302663 cu. ft.
Fraction of the space displacement of the piston per stroke in clearance and steam passages at one end of cylinder, . . .	0·024647
Ratio of length of connecting rod to length of crank, between centres, . . .	5·
Face of piston, . . .	$6\frac{1}{4}$ inches.

Aggregate width of the two packing rings in the piston, ...	$1\frac{1}{2}$ inches.
Width in clear between the cylinder and its steam jacket, ...	$3\frac{1}{4}$ inches.
Thickness of metal of cylinder, . .	$1\frac{1}{4}$ inches.
Thickness of metal of jacket, . .	$1\frac{3}{8}$ inches.

The sides and ends of the cylinder were steam jacketed, the jackets being cast in one piece with the cylinder, and with its ends, both of which latter were bolted on. All the jackets communicated and were drained of their water of condensation by a pipe with an automatic valve, and also by a valve to be worked by hand at will.

The piston was steam jacketed as well as its cylinder, that is to say, it was cast hollow and had its interior filled with steam through a horizontal tube of $\frac{7}{8}$ inch interior and $1\frac{1}{4}$ inch exterior diameter, one end of which was screwed into the piston as near as possible to the upper end of its vertical diameter. This tube passed through one end of the cylinder, by means of a stuffing-box, into a receptacle or closed case outside of the cylinder, by means of another stuffing-box. The case was filled with steam directly from the jackets of the cylinder, through a small pipe, and from the case the steam passed through the tube into the interior of the piston, the tube moving with the piston forward and backward, and passing air and steam-tight through the two stuffing-boxes. By a precisely duplicate arrangement, the water of condensation was drained from the interior of the piston, the horizontal drain tube being screwed into the piston as near the lower end of its vertical diameter as possible, passed through a stuffing-box in the end of the cylinder, and then through another stuffing-box in the end of a case or receptacle; the water of condensation being thus discharged into the case was thence carried off by a small vertical pipe, fitted with a stop cock.

The piston rod, with uniform diameter, extended through the piston, and through both ends of the cylinder by means of stuffing-boxes.

The cylinder had four valves, two for the steam and two for the exhaust; the former were at the highest point of the cylinder, the latter were at the lowest. They were cylindrical and worked in cylindrical seats with a vibrating motion. The steam valves were also the cut-off valves, the point of cutting off being variable and controlled by the governor of the engine, which acted directly on them.

All the steam jackets were supplied with steam directly from the boiler, and had no connection with the valve chests or cylinder.

The engine was fitted with a jet condenser placed beneath the cylinder. The air-pump was horizontal and its piston received its movement from a vertical lever or half beam supported in a pillow block at its lower end. This half beam was worked by means of a short link connecting it with the crosshead. The feed pump was also horizontal and operated in the same manner.

The cylinder with which the experiments were made was supplied with saturated steam by a boiler which was exclusively employed for that purpose. Another boiler supplied the other cylinder, and between these boilers there was no communication. The experimental cylinder was connected to its boiler by a steam pipe 80 feet in length and 6 inches in outside diameter. This pipe inclined strongly upwards after leaving the top of the steam drum of the boiler, so that any water entrained into it from the boiler by the steam, or any water of condensation in it due to external refrigeration, would run back to the boiler by gravity.

The cylinder and steam pipe were well protected from losing heat by radiation, being covered with non-conducting material.

The cylinder valves had neither steam lead, nor exhaust lead, nor cushioning.

The Experiments.

The main purpose of the committee was to ascertain for the engine in question the cost of the indicated horse-power in pounds of feed water consumed per hour when no steam was present in either the jackets or in the piston of the cylinder; when steam was present in the cylinder jackets but not in the piston; and when steam was present both in the cylinder jackets and in the piston. Throughout all the experiments the piston speed and the boiler pressure were to be maintained as nearly constant as possible, with the throttle valve wide open; but in each of the three cases the engine was to develop different indicated horses-power by cutting off the steam at different fractions of the stroke of the piston, the constant speed of the piston being maintained by the help of the duplicate engine connected to the same shaft. The determinations of the cost of the indicated horse-power in the different cases, and for the same number of horses-power developed, gave the economy due to the presence of steam in the cylinder jackets alone, and in the cylinder jackets and piston combined.

The experiments made with the same piston speed, boiler pressure, and position of throttle valve, either with or without steam in the jackets and piston of the cylinder, gave, incidentally, the economical effect, under strictly comparable conditions, of the different measures of expansion with which the steam was used, though this determination was no portion of the intention of the committee, who, in their conclusions, utterly ignore any difference in the economic result of the different measures of expansion, assuming, apparently, the economy to be the same with all.

Twelve experiments were made: three without steam in either the cylinder jackets or piston; one, accidentally, with the jackets partly filled with steam and partly filled with the water of its condensation, owing to the incomplete drainage of the latter; two with steam in the jackets of the cylinder but not in the piston; and six with steam in the cylinder jackets and piston. The measure of expansion with which the steam was used varied from 5·7037 to 12·3978 times. The boiler pressure averaged 68·2480 pounds per square inch above the atmosphere, and the number of double strokes made by the piston averaged 49·8925 per minute for the different experiments. The steam pressure in the valve chest of the cylinder averaged 3·5 pounds per square inch less than in the boiler, and the steam pressure in the cylinder at the commencement of the stroke of the piston averaged 6·827 pounds per square inch less than in the valve chest, owing to the smallness of the steam ports, making the pressure on the piston at the commencement of its stroke 10·327 pounds per square inch less than in the boiler. This considerable difference of pressure must have given the steam on entering the cylinder about 8·5 degrees Fahrenheit of superheating.

Each experiment was intended to continue one working day of the factory, and such was the case with nine of them, which averaged 10·6371 hours; the remaining three, however, owing to accidents, continued only half days each, averaging 5·4765 hours.

A set of indicator diagrams, and a complete set of observations of the steam pressures in the boiler and in the valve chest of the cylinder, of the back pressure in the condenser, and of the temperatures of the injection and feed water, were taken every fifteen minutes. The temperature of the injection water was sensibly constant throughout at 50¼ degrees Fahrenheit.

Two indicators were employed, and had their springs carefully

tested at the temperature of use. They remained permanently in position, one at each end of the cylinder, and gave very consistent diagrams. The number of double strokes made by the piston was taken by a counter.

The feed water was delivered by the air-pump through a special pipe into two tanks, alternately, and its quantity therein ascertained. Each tank contained 106 cubic feet. The weight of water per tank was ascertained by weighing into the latter water of the temperature of the feed in quantities of 220 pounds at a time, and then repeating the weighing by drawing off the water as a check; a gauge placed at the side of the tank was thus graduated to intervals of 2·2 pounds, so that at any moment the exact quantity in the tank could be known by inspection. The cost of the power was measured by the pounds of feed water consumed per hour per horse-power.

The water of condensation from the jackets of the cylinder, and from the piston, were drained into small tanks and weighed separately from time to time during the experiments in quantities of 22 pounds.

The pressures were taken from carefully tested manometers. The observers were numerous, and each had his special observations to make. Every provision was made to secure strictly accurate results.

An experiment was made to ascertain by the indicator the pressure on the steam piston required to work the engine, *per se*, that is to say, the unloaded engine; the factory shafting being uncoupled for this purpose, so that the experiment determined the piston pressure absorbed by the friction of the engine, gearing and shafting up to the coupling when working with the shafting and looms of the factory disconnected.

After the shafting was uncoupled, the engine thus unloaded was worked thirty minutes at 52·102 double strokes of its piston per minute, during which time sixteen indicator diagrams were taken whose mean gave the pressure 2·1641 pounds per square inch of piston. As the resistance of the engine, gearing and shafting under these conditions was almost wholly frictional, and therefore uninfluenced by the speed of the piston, this pressure remains constant at all speeds of piston, and must be deducted from the indicated pressure when the engine is loaded in order to give the net pressure applied to the crank pin.

In addition to the above determination of the piston pressure required to work the unloaded engine, gearing and shafting up to the

coupling, the committee made another determination by means of two friction brakes placed on the pinion shaft as close to the coupling as practicable. From these brakes were ascertained the horses-power transmitted to the shafting beyond them, while the indicator gave simultaneously the indicated horses-power exerted by the engine. The subtraction of the first power from the last gave the power absorbed by the friction of the engine, the friction of the gearing, and the friction of the shafting up to the coupling, *per se*, and *also the friction produced by the load on these mechanisms*; consequently, the power obtained by the subtraction of the brake power from the engine power was greater than that due to working the unloaded mechanisms by the amount due to the friction produced by the load upon them. The friction resistances of an engine are of two kinds: one, the friction of the unloaded engine, due simply to the weight of its moving parts, and to the pressure of the packings of the pistons, piston-rods and valves; this friction is constant at all speeds of piston, and is wholly independent of the load, be the latter great or small; the pressure on the steam piston required to overcome it is the true pressure required to work the unloaded engine, *per se*, and should always be used to obtain the net pressure upon the crank pin by subtracting it from the indicated pressure. The other friction resistance of an engine is that which is due simply to the load, and is a constant proportion of the load, varying directly as the latter varies. Now the load is represented by the pressure on the crank pin, or net pressure, that is to say, by the difference of the indicated pressure and the pressure required to work the unloaded engine, so that the friction due to the load is a constant fraction of the net pressure or pressure on the crank pin, which pressure with the same engine is variable within very wide limits. It is evident that when the brakes were in action, the difference between the power shown by them and the indicated power exerted by the engine, included both kinds of friction resistance, for the load in that case was upon the engine, and the friction of the load had to be overcome as well as the friction of the mere weight of the mechanism and of the pressure of the packings. The only method by which the friction of the unloaded engine can be obtained is by throwing off the entire load and ascertaining by the indicator the pressure required to work the mechanism, *per se*.

The experiments with the brakes continued one hour and thirty-eight minutes, during which sixty-nine indicator diagrams were taken.

The mean speed of the piston was 49·64 double strokes per minute, from which the extremes varied by only one-third of a double stroke; and the pressure on the piston, due to the horses-power obtained by subtracting the brake power from the indicated power exerted by the engine, was 2·3452 pounds per square inch. The pressure due to working the unloaded mechanism, as given direct by the indicator, was 2·1641 pounds per square inch of piston, consequently the difference between that pressure and the pressure of 2·3452 pounds was due to the friction of the load, and is $\left(\frac{2{\cdot}3452 - 2{\cdot}1641 \times 100}{2{\cdot}3452} =\right)$ 7·722 per centum of the load. The friction coefficient due to the load alone has always been assumed by the writer for engines as habitually lubricated at 7·5 per centum of the load.

The data and results of the experiments will be found in the following table, carefully changed from the French into English measures. The data are the totals and means of all the indicator diagrams and of all the observations taken. This table contains all the reliable facts which can be obtained from the text, the tables and the indicator diagrams given in the report of the committee. That report is very meagre, leaving much to be supplemented and inferred, and were it not for the diagrams reproduced—one as a representative for each experiment—from which some important quantities could be taken, the experiments would lose much of their value. It is to be regretted that, with so convenient an arrangement of engines, and after such elaborate preparations, so few experiments were made and so few facts of interest reported. Even as regards the experiments actually made, some most important data are omitted. For instance: the steam pressures in the cylinder at the commencement of the stroke of the piston, at the point of cutting off the steam, and at the end of the stroke of the piston, are not given; neither is the mean back pressure against the piston, nor the back pressure against it at the commencement of its stroke; nor is even the indicated pressure given, nor the fraction of the stroke of the piston at which the steam was cut off. All these could be obtained from the indicator diagrams actually taken. What is given from them are only the indicated horses-power developed in the different experiments, and the horses-power required to work the unloaded engine, and from these the writer has calculated the indicated and net pressures in the different cases, while from the

representative diagrams he has taken the back pressure against the pistons and the point of cutting off the steam.

The pressure on the piston at the commencement of its stroke must be stated, and also the mean back pressure against it, in order that the pressures between which the engine worked may be known. The latter pressure is necessary, too, for ascertaining the total pressure on the piston—a most important quantity. The pressures at the point of cutting off the steam, and at the end of the stroke of the piston, together with the back pressure against the piston at the commencement of its stroke, and the point at which the steam was cut off in fractions of the stroke, are necessary for calculating the weight of steam present at those points in the cylinder, from which and the weight of water pumped into the boiler the cylinder condensation can be known, one of the most valuable of determinations and the only one which explains the economy derived from steam jacketing whose effect without such explanation is merely mysterious. A properly made set of experiments, and a proper report of them, not only gives *all* the facts in relation to the subject, but furnishes an elucidation of them, showing why they are as they are, and pointing out the producing and modifying causes. Had the committee submitted a simple table containing all the quantities it was possible to observe, and just as observed without corrections or deductions, others would have been enabled to make the necessary calculations and inferences which are now impossible. Nothing is more unsatisfactory than a report in which only the conclusions of the experimenters are given without a statement of the complete experimental facts and processes on which they are based. The experimental results in steam engineering are not absolute, but relative entirely to the mechanism and methods employed; hence the necessity for a complete description of both and a clear statement of all the quantities involved, whether bearing on the immediate views and purposes of the experimenters or not.

The committee in their report do not give the weight of feed water actually pumped into the boiler, nor the weight of the water of condensation actually drained from the steam jackets and piston of the cylinder; but they give these weights reduced to the temperature of 32 degrees Fahrenheit. That is to say, taking the product of the total heat imparted to the feed water in the boiler above its actual temperature on entering, into the the actual weight of that water, they

calculated what would be the weight of water given by dividing this product by the total heat that would have to be imparted to the feed water had its temperature on entering the boiler been 32 degrees Fahrenheit instead of the temperature it actually was, in order to convert it into steam of the boiler pressure. This quotient is a factitious quantity and misleading. It is not the weight of steam producing the power; that weight is independent of the temperature of the feed water and would be the same, other things equal, let the temperature of the feed water be what it might. The committee's calculated weight of water only represents relatively the units of heat that would have been imparted to the feed water on the supposition that its temperature was 32 degrees Fahrenheit, a perfectly useless piece of knowledge; what is required to be known are the units of heat that were actually imparted under the experimental conditions to the feed water. Different mechanisms and methods of using steam are necessarily accompanied by inseparable different temperatures of the feed water, and these are the temperatures which should furnish the bases for computation and not factitious or arbitrary ones. Accordingly, the writer has been obliged to re-obtain the true or experimental weights of feed water by taking the committee's factitious weights in the different experiments, and reversing the order of their calculations, employing in each case the experimental temperatures of the feed water and of the boiler steam; and these weights are given in the following table instead of the erroneous weights calculated by the committee. Nothing like this table will be found in the committee's report, but it contains all the facts which could be collected from the text, tables and indicator diagrams therein given.

For facility of reference, the quantities in the table have been arranged in appropriate groups: the description of the quantities on their respective lines is so full that no additional explanations are needed, save that the net pressures on the piston are in all cases the indicated pressures less 2·1641 pounds per square inch, which is the pressure required to work the unloaded engine; and that the total pressures on the piston are in all cases the sum of the indicated pressure on the piston and of the back pressure against it. Each experiment is designated by a capital letter placed at the head of the tabular column containing its data and results.

(To be continued.)

ORLISS CONDENSING STEAM ENGINE DRIVING THE FACTORY OF MESS
IONS OF NO STEAM IN THE CYLINDER JACKETS NOR IN THE PISTON; O
STEAM IN THE PISTON; OF THE CYLINDER JACKETS FILLED WITH STE
ND PISTON FILLED WITH STEAM OF BOILER PRESSURE.

. *The throttle valve was wide open throughout, and the steam used was satura*

n jackets of the linder not thoroughly drained, t filled partly th boiler steam l partly with its ter of condension. steam in the am-piston.	Boiler steam in the steam jackets of the cylinder, but no steam in the steam-piston.		Boiler steam in the steam jackets		
D	*E*	*F*	*G*	*H*	*I*
April 10th.	May 6th, P.M.	April 11th.	May 7th.	May 8th.	May 11t
10·6625	5·0708	10·7583	10·8740	10·5040	9·65
34813·0193	10114·8081	34000·8126	21804·5669	27601·1743	28342·06
1387·0796	512·2794	1123·4859	1116·9229	1521·5963	1173·635
....	....	....	292·5192	538·2278	351·192
....	....	....	1409·4721	1062·8241	1524·827
3·9844	5·0647	4·1866	5·1224	4·7990	4·111
....	....	....	1·3417	1·2254	1·250
....	....	....	6·4641	6·0244	5·580
31969·	15324·	32253·	32882·	32480·	28817·
67·7000	68·1557	66·8472	67·8854	67·1316	70·758
63·2900	64·7422	63·4195	64·5900	63·5484	67·960
Wide.	Wide.	Wide.	Wide.	Wide.	Wide.
0·1550	0·0700	0·1550	0·0700	0·1050	0·125
5·7037	10·8259	5·7037	10·8259	7·9033	6·847
2·1648	1·9718	2·1648	1·9718	2·1069	2·126
84·	79·	84·	79·	82·	83·
3264·9959	1994·7164	3160·4289	2005·2020	2605·3871	2025·127
49·9711	50·3668	49·9661	50·3985	51·0981	49·58
71·1510	72·6030	71·2800	72·4510	71·2090	77·800
3·2472	2·9577	3·2472	2·9577	3·1599	3·189
29·3373	18·5252	29·5856	19·1925	21·5181	27·845
27·1732	16·3611	27·4215	17·0284	22·3540	25·081
32·5845	21·4829	32·8328	22·1502	27·6780	31·031
157·0947	100·3357	158·1246	103·0811	131·2555	147·950
145·5213	88·3128	146·8363	91·9725	122·4127	135·470
174·2606	115·9589	175·8127	119·3060	151·5074	164·022
20·7836	19·8804	19·9491	19·6158	19·4080	19·748
22·4365	22·5860	21·5235	21·8022	21·2876	21·418
18·7363	17·2419	17·9761	16·8050	17·1896	17·724
23395·7265	22481·7424	22451·2258	21873·0947	21879·1890	22010·65
25256·3664	25542·5868	24223·0657	24652·9123	23997·6122	24101·780
21091·1175	19452·7617	20230·7613	19000·0725	16813·5170	20018·152

STEAM JACKETING.

By Chief Engineer ISHERWOOD, U. S. Navy.

Table containing the Data and Results of the Experiments made at Mulhouse, in Alsace, Germany, on the Jacketed CORLISS Condensing Steam Engine driving the Factory of Messrs. Schlumberger, Son & Co., to determine its Economic Efficiency in rapport of Power to Weight of Steam Consumed under the different conditions of no steam in the cylinder jackets nor in the piston; of the cylinder jackets partly filled with steam of boiler pressure and partly filled with its water of condensation, but no steam in the piston; of the cylinder jackets filled with steam of boiler pressure, but no steam in the piston; and, finally, of the cylinder jackets and piston filled with steam of boiler pressure.

The piston speed and boiler pressure were nearly constant, but the steam was used with different measures of expansion. The throttle valve was wide open throughout, and the steam used was saturated.

		No steam in the steam jackets of the cylinder, nor in the steam-piston.			Steam jackets of the cylinder not thoroughly drained, but filled partly with boiler steam and partly with its water of condensation. No steam in the steam-piston.	Boiler steam in the steam jackets of the cylinder, but no steam in the steam-piston.		Boiler steam in the steam jackets of the cylinder, and in the steam-piston.					
		A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*
	Date of the experiments,	May 6th, A.M.	April 8th.	April 9th.	April 10th.	May 6th, P.M.	April 11th.	May 7th.	May 8th.	May 11th.	May 10th.	May 9th.	April 12th
TOTAL QUANTITIES.	Duration of experiments in consecutive hours,	5·6814	10·7080	5·6772	10·6625	5·0708	10·7583	10·8740	10·5940	9·6958	10·9080	10·8000	10·7334
	Total number of pounds of feed water pumped into the boiler,	13089·5382	35550·5679	18725·6027	34813·0193	10114·8081	34000·8426	21804·5669	27601·4743	28342·0635	32855·5257	33087·7010	35284·5109
	Total number of pounds of water of condensation drained from the steam jackets of the cylinder,	...	...	...	1387·0796	512·2794	1423·4869	1116·9229	1324·5963	1173·6358	1327·3678	1304·9939	1275·7276
	Total number of pounds of water of condensation drained from the steam-piston,	...	...	...	...	...	...	292·5492	338·2278	351·1921	349·8886	370·2830	349·1283
	Aggregate number of pounds of water of condensation drained from the steam jackets of the cylinder and from the steam piston,	...	...	...	...	...	...	1409·4721	1662·8241	1524·8279	1677·2564	1675·2769	1624·8559
	Per centum of the steam evaporated in the boiler condensed in the steam jackets of the cylinder,	...	...	...	3·9844	5·0647	4·1866	5·1224	4·7990	4·1410	4·0400	3·9440	3·6155
	Per centum of the steam evaporated in the boiler condensed in the steam-piston,	...	...	...	...	...	...	1·3417	1·2254	1·2391	1·0646	1·1171	0·9895
	Aggregate per centum of the steam evaporated in the boiler condensed in the steam jackets of the cylinder and in the steam piston,	...	...	...	...	...	...	6·4641	6·0244	5·3801	5·1046	5·0611	4·6050
	Total number of double strokes made by the steam-piston,	17019·	31605·	16762·	31969·	15324·	32253·	32882·	32480·	28847·	32358·	31978·	32333·
ENGINE.	Steam pressure in boiler, in pounds per square inch above the atmosphere,	67·7148	66·2072	67·1601	67·7000	68·1557	66·8472	67·8854	67·1316	70·7584	70·7000	71·3558	67·3592
	Steam pressure in valve chests of cylinder, in pounds per square inch above the atmosphere,	64·1022	62·7072	63·6600	63·2900	64·7422	63·4195	64·5900	63·3484	67·9992	67·2027	67·7574	63·5475
	Proportion of throttle valve open,	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.	Wide.
	Fraction of stroke of piston completed when the steam was cut off,	0·0580	0·1050	0·1050	0·1550	0·0700	0·1550	0 0700	0·1050	0·1250	0·1350	0·1500	0·1550
	Number of times the steam was expanded,	12·3978	7·9033	7·9033	5·7037	10·8259	5·7037	10·8259	7·9033	6·8471	6·4182	5·8670	5·7037
	Pressure in the condenser in pounds per square inch above zero,	1·9907	2·0680	2·0880	2·1648	1·9718	2·1648	1·9718	2·1069	2·1260	2·1260	2·1648	2·1844
	Temperature, in degrees Fahrenheit, of the feed water,	80·	83·	83·	84·	79·	84·	79·	82·	83·	84·	85·	85·
	Number of pounds of feed water pumped per hour into the boiler,	2303·9283	3320·0007	3298·3870	3264·9959	1994·7164	3160·4289	2005·2020	2605·3874	2923·1279	3012·0577	3063·6760	3287·3564
	Number of double strokes made per minute by the piston,	49·9261	49·1922	49·2085	49·9711	50·3668	49·9661	50·3985	51·0981	49·5868	49·4408	49·3488	50·2062
STEAM PRESSURES IN CYLINDER.	Steam pressure on piston at commencement of its stroke, in pounds per square inch above zero,	71·9630	70·5680	71·5210	71·1510	72·6030	71·2800	72·4510	71·2090	75·8600	75·0646	75·6180	71·4080
	Mean back pressure against piston during its stroke, in pounds per square inch above zero,	2·9860	3·1020	3·1320	3·2472	2·9577	3·2472	2·9577	3·1599	3·1890	3·1890	3·2472	3·2766
	Mean indicated pressure on piston during its stroke, in pounds per square inch,	15·2113	24·0281	24·3657	29·3373	18·5252	29·5856	19·1925	24·5181	27·8458	28·7398	29·5327	29·9900
	Mean net pressure on piston during its stroke, in pounds per square inch,	13·0472	21·8640	22·2016	27·1732	16·3611	27·4215	17·0284	22·3540	25·6817	26·5757	27·3686	27·8259
	Mean total pressure on piston during its stroke, in pounds per square inch,	18·1973	27·1301	27·4977	32·5845	21·4829	32·8328	22·1502	27·6780	31·0348	31·9288	32·7799	33·2666
HORSES-POWER.	Indicated horses-power developed by the engine,	81·3881	126·6726	128·4950	157·0947	100·3357	158·4246	103·6611	134·2635	147·9764	152·2772	156·1874	161·3620
	Net horses-power developed by the engine,	69·8090	115·2637	117·0824	145·5213	88·3128	146·8363	91·9725	122·4127	136·4761	140·8108	144·7423	149·7180
	Total horses-power developed by the engine,	97·3647	143·0258	145·0219	174·2606	115·9589	175·8127	119·3660	151·5674	164·9232	169·1740	173·3606	178·9918
ECONOMIC RESULTS.	Number of pounds of feed water consumed per hour per indicated horse-power,	28·3079	26·2093	25·6694	20·7836	19·8804	19·9491	19·3438	19·4050	19·7540	19·7801	19·6154	20·3725
	Number of pounds of feed water consumed per hour per net horse-power,	33·0033	28·8035	28·1715	22·4365	22·5869	21·5235	21·8022	21·2836	21·4186	21·3908	21·1664	21·9570
	Number of pounds of feed water consumed per hour per total horse power,	23·6629	23·2126	22·7441	18·7363	17·2019	17·9761	16·8030	17·1896	17·7242	17·8045	17·6723	18·3659
	Number of Fahrenheit units of heat consumed per hour per indicated horse-power,	31979·2535	29518·5124	28916·7922	23395·7265	22481·7424	22451·2258	21873·0947	21879·1860	22310·6595	22280·4747	22078·5215	22910·1149
	Number of Fahrenheit units of heat consumed per hour per net horse-power,	37285·6168	32440·2587	31735·4286	25256·3664	25542·3868	24223·0957	24652·9423	23997·3122	24190·7002	24094·7811	23824·2818	24691·9815
	Number of Fahrenheit units of heat consumed per hour per total horse-power,	26731·8267	26143·4461	25621·4174	21091·1175	19452·7617	20230·7613	19000·0729	19381·3170	20018·1528	20055·4419	19891·4248	20653·5712

REPAIRING A BROKEN CRANK WITH WIRE ROPE.

The following description of the temporary repairs made to the broken cranks of the U. S. steamer *Pensacola* by G. W. Stivers, Passed Assistant Engineer U. S. Navy, is interesting in showing what may be done with wire rope in the way of repairs on shipboard.

The after crank of the forward engine only is shown, this one having been broken entirely across; but the plan adopted in banding both cranks of the after engine, which were broken nearly across, was precisely the same. The crank shaft and cranks were in one solid forging, and the fractures were directly across the bodies of the cranks, between the crank pins and the shaft, as if following the plane of contact of the fagots used in building up the cranks in the smithery.

For several months the existence of a very slight crack in the after crank of the forward engine was known, but frequent examinations failed to show any extension, and it was considered simply a surface flaw of no great consequence.

At about 5 P.M., June 18th, 1880, the ship being on her way from San Francisco to Puget Sound, it was found that this crack was rapidly extending. The weather was fine, sea smooth, and with square sails set the engines were slowed down, while preparations were made to strengthen the crank by temporary bands, so as to reach Victoria, where it was expected that a heavy band could be forged and shrunk on. The only iron that could be worked on board ship was a piece $3\frac{7}{8}$ inches wide and $\frac{3}{4}$ inch thick, and it was proposed to make of this two bands, to embrace the crank side by side and to be set up by two 1-inch bolts on each side, passing through lugs formed by bending up and thickening the ends. The strength of the bands as straps was therefore only that of two 1-inch bolts, and its utter insufficiency was evident.

At about 9 o'clock that night the crank had broken entirely across, but as the two parts moved together as usual, the irregular surfaces of the break and the long forward journal preventing their separation, the engines were kept slowly revolving until noon of the next day, when the plan of iron bands was abandoned, before being carried out, in favor of the following one, proposed by Mr. Stivers.

Ten gutter pieces or shoes were made of an old iron grating and placed equidistant about the crank in order to keep the bottom turns of wire rope from spreading apart when the riding turns came to be put on over them. The shoe at the extreme end and that on the butt of the crank only were secured in place by a small bolt through the centre, the other shoes being held in proper position by hand until bound by the first turn or two of rope. Into the hub of the crank a bolt one inch in diameter was tapped, to which the end of the rope was secured, and the work of wrapping the crank consisted simply in passing the wire rope continuously around the crank, hauling each turn taut by means of a heavy deck tackle passing down through the engine room hatch, and seizing each turn securely to the one immedsately preceding it before slacking off the tackle. It was estimated that a tightening pull of about two tons was brought on each turn of rope, and the seizing of the separate turns having been done very expertly there was no slacking back of the wire rope during the progress of the whole work.

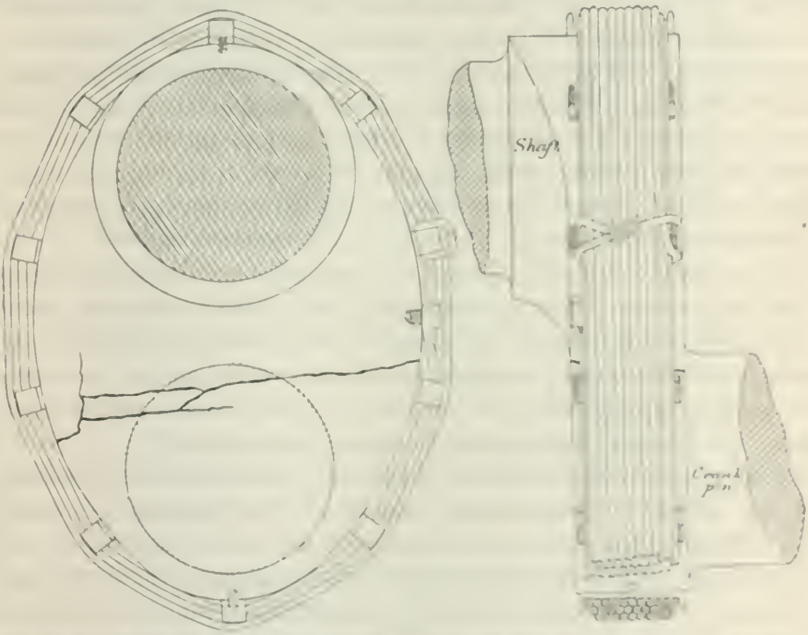
In this manner eleven turns of the rope were put around the crank side by side, just filling up the space between the jaws of the shoes; then over these were put ten riding turns, and over these again nine riding turns, making altogether thirty turns of wire rope encircling the crank. The finishing end was simply passed around the banding at one of the shoes and stopped, as shown in the sketch.

The wire rope used in this work was of $\frac{9}{16}$ inch diameter, consisting of five strands of 17 wires each, arranged about a central hemp core of $\frac{3}{16}$ inch diameter, the wires being of number 19 wire gauge.

The work of banding this crank was completed in seven hours, and just as it was finished it was found that both the cranks of the after engine were broken nearly across, the cracks extending entirely through each crank to within about one inch of each side, in about the same direction and relative position as in the forward crank. This discovery caused the abandonment of the attempt to reach Victoria; the ship was headed to the southward, with the intention of returning as best we could to San Francisco under sail, and, the banding of the forward crank having been so satisfactorily accomplished, it was decided to strengthen the after cranks in the same manner, so that the engines might be used in case of emergency.

This was done, and when about 200 miles from San Francisco, the wind blowing lightly from ahead and sails being useless, the engines

were started and worked continuously without stopping for over 45 hours, until the anchor was dropped in the harbor of San Francisco. During this time 45,835 revolutions were made, an average of 16·85 revolutions per minute, and an average speed made of 4·41 knots per hour under steam alone. The next day the ship was steamed up to the Mare Island Navy Yard, 26 miles from San Francisco, the engines being stopped and started seventeen times during the run to allow target practice, and upon arrival at the yard the rope banding was found to be as firm and unyielding as when first put on the cranks.



Arrangement of Wire Rope Banding, as applied to the fractured cranks of the U. S. S. *Pensacola*. Designed by Geo. W. Stivers, Passed Assistant Engineer U. S. N.

The speed at which the engines were run with these fractured cranks was low, for the weather was fine and no necessity required the taking of the least risk; the initial pressure above zero on the pistons was 20 pounds, cutting off at 9 inches, there being two cylinders 60 inches diameter and 36 inches stroke; yet it is evident from a calculation of the strength of the banding on the extreme after crank, which transmits the power of both engines, and which would have been further strengthened if more wire rope had been at hand, that a considerably greater speed could have been safely attained.

The gutter pieces or shoes were, it will be seen, indispensable to the successful application of the wire rope banding, for they prevented the first course of rope from spreading under the pressure of the riding turns; they allowed the driving of iron wedges beneath them without injury to the rope, in case it should become slack and require tightening, and they permitted the passing of the end of the rope between the crank and the whole banding so as to secure it firmly.

It was expected that the wire rope would in a short time stretch, and the intention was therefore to first drive iron wedges under each shoe, nipping the ends over the end of the crank, and then to drive all around, under the rope banding, dry pine wedges which would be swelled by the water used on the crank pins, but there was no necessity for any wedging whatever, and perhaps would not have been had the engines been in operation a much longer time than they were. The shape of these cranks made the application of the banding quite a simple matter, though it would not be difficult to adopt such a system of wrapping with wire rope to a crank of any shape whatever.

It is thought that the idea has not occurred to engineers generally that they have in wire rope such a ready and effective means of temporarily repairing damages to engines at sea. Many repairs that are now made with straps or bands hastily constructed under the disadvantages always attending smith's work on board ship can be much more quickly and easily made by a few turns of wire rope. This material may yet be the means of repairing a broken shaft of a steamer so that she will be able to steam into port at a moderate rate of speed.

In the case of repairs to a broken shaft the ends of the rope could be passed through and secured to the back of square pieces fitted into keyways cut in the shaft, then passing the rope around the broken shaft and securing the other end of the rope in a like manner; these pieces to be bolted to the shaft with tap bolts; for backing the rope must be wound around the shaft in the opposite direction.

The ease and facility with which repairs can be made with wire rope commend it to engineers and steamship owners as a part of the necessary outfit for a steamship. J. C. KAFER.

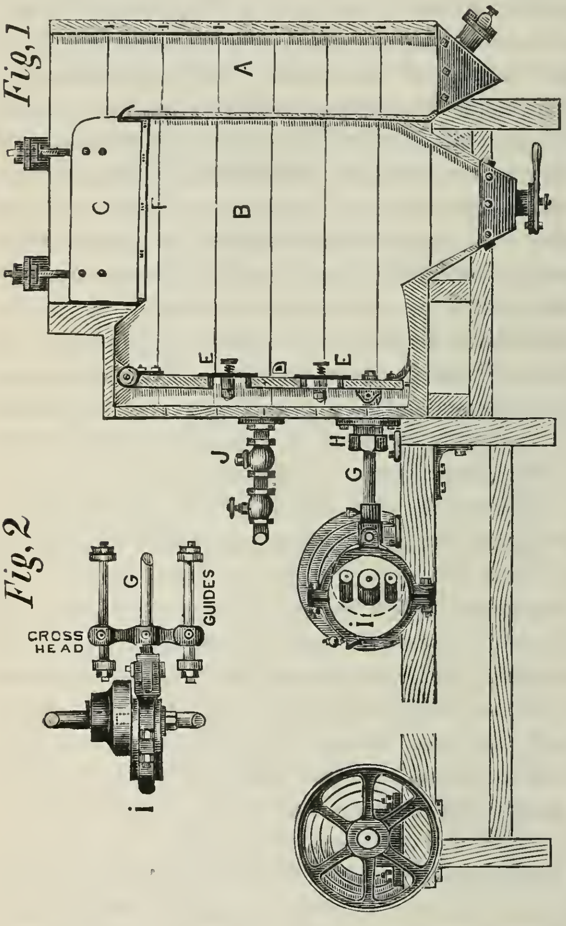
Magic Well.—An artesian well at Atchison, Kansas, furnishes both fresh and salt water. A long tube, which reaches to the bottom of the well, penetrates a salt vein, while a shorter one only extends to a stream of fresh water.—*Fortsch. der Zeit.* C.

CONCENTRATION OF LOW GRADE ORES.

The large deposits of low grade ores that are being continually uncovered in the search for the ores of higher grade place a question before the mining engineer and metallurgist which has claimed much attention in the past few years. The low grade ores are those which run very low in metallic substance in proportion to their gangue. That is a low grade ore which runs from $5 to $35 per ton. If it were necessary to work a low grade ore, to roast, pulverize, amalgamate and retort it would cost so much, with the additional expense of mining and hauling that it would not pay to touch it without concentration; concentration is dividing the metallic ore from the waste rock. This is done in some countries by pounding the ore into small pieces and, by hand, picking the good out of the waste; this was the original separation. The next step was to fill a tank with water, suspend a smaller box in it having a screen bottom and being fastened to a long pole, which was pivoted on an upright post. The ore, broken into small pieces, was then put into this box, the end of the pole grasped and a quick up and down movement given to the pole and box. This quick plunge of the smaller box, containing the ore, into the water in the large box pushes the quartz or rock up and allows the metal, being heaviest, to settle at the bottom of the box. The rock, being lighter than the metal, collects upon the surface and is skimmed off. This was the first mechanical means used for the separation of ore. From this simple machine has grown the present concentrator, now in use in England and the United States.

The following cut represents the Rogers Swing Gate Concentrator, designed especially to meet the requirements of the mining engineer in working the low grade ores successfully and profitably. As the concentration of ore depends upon the pulsation of water in the gig box, where the ore is separated from its gangue, light ores require but little agitation and heavy ores greater agitation. This has long been known as a great point in concentration, but there has never been a machine constructed with this principle in view, so that any ore, either of great or light specific gravity, could be treated until the present one, represented in the cut. This is the only concentrator which is capable of treating *all ores*. It is recommended on account of its cheapness,

simplicity and capability of working any ore which will separate from its gangue.



These machines consist of a water tank divided into two compartments, A and B; one for waste, A; and one for concentration, B. In the main compartment is suspended the box C by four bolts, passing through brackets; these bolts are for the purpose of leveling the screen F and for altering its height, in order to get more or less bed. In the main compartment, B, is suspended the vibrating gate D, provided with valves E E. These valves are opened and closed by the variation in the pressure of the body of water on either side of them. This pressure of the water is rendered intermittent by the backward and forward swing of the gate to which they are affixed. The forward swing of the gate moves the large body of water in advance of it upward through the meshes of the screen, while at the same time a partial vacuum is produced in the smaller body of water behind the gate. The check valve J, connected with a suitable reservoir of water, then opens and supplies this deficiency. The vibratory motion or swing of the gate is obtained by means of a rod, G, attached to the lower part of the gate, passing through the stuffing-box H and moved by the adjustable eccentric I.

In cases where water is scarce these concentrators can have a pipe, protected with a screen nozzle, projecting into the waste compartment, A, and running into the main compartment, B, where a check valve allows of an intermittent stream being pumped, by the action of the vibrating gate, through from the waste and forcing it up through the screen. By this simple arrangement the water can be used over and over again.

It will be seen at a glance that this concentrator is so perfectly adjustable in all its working parts as to allow of its being run by

unskilled labor. Another great desideratum is its capability to treat ores of greater or less specific gravity merely by altering vibration of the gate, by means of the adjustable eccentric, thereby producing either a slight or heavy pulsation of water through the screen. Any desired speed can be obtained by means of the cones. The tight and loose pulleys admit of instant stoppage, without interfering with any of the pulleys or shafting in the mill. Extra castings for the screen grates are provided with each machine, so that different sized screens, from 4 to 80 mesh, can be placed in position in a few minutes, causing very little delay in the work. The weight of each machine, tank included, is about 850 pounds, and each machine can be shipped in sections, and put together at the mines from the construction drawings.

Six of these concentrators are already in successful operation at Alpine, Colorado, by the Pembina Mining and Milling Company, where low grade ores are being worked with the best results possible.

APPROXIMATE QUADRATURES OF THE CIRCLE. IV.

By Pliny Earle Chase, LL.D.

A note from E. Dietze, C.E., calls attention to the following formula, which he thinks was published in the *Leipziger Illustrirte Zeitung* in the fall of 1856.

$$\pi = \tfrac{1}{4}\sqrt{5} + \sqrt{21} - 2.$$

The formula may be easily constructed, for

$$2\,r\pi = \sqrt{r^2 + \left(\frac{r}{2}\right)^2} + 2\left[\sqrt{(5r)^2 - (2r)^2} - 2r\right]$$

It gives $\pi = 3{\cdot}1415926893$, the true value being
$3{\cdot}1415926539$

Error, $\cdot 0000000354$

This error is only $\frac{2}{15}$ as great as in the sixth approximation of my note.* The construction is much easier and simpler than that of Perkins, but it is more difficult than either of my own.

* This Journal, June, 1880, p. 409.

Frozen Dynamite.—Captain George Lebon recommends the use of an increased quantity of fulminate in dynamite cartridges, so that they can be readily exploded when frozen, thus obviating the necessity and consequent danger of melting them in cold weather. He mentions numerous experiments, at temperatures of twenty degrees below zero (—4°F.), in which charges were exploded without difficulty or failure.—*Ann. des Ponts et Chauss.* C.

Wormwood as an Insectifuge.—M. Poyrot having observed that the immense tracts of wormwood, upon the American plains, are free from insects of every description, is experimenting with the plant as a preventive of phylloxera. He finds no difficulty in cultivating the wormwood, and he proposes to mix the stalks with manure, or simply bury them in the ground in the neighborhood of the vines. His suggestions have been sent to the phylloxera committee of the French Academy.—*Comptes Rendus.* C.

Aluminium as a Voltaic Element.—Aluminium, like iron, has the remarkable peculiarity, when immersed in concentrated nitric acid and then placed in contact with ordinary aluminium, of exciting a galvanic current. It is thus possible to construct a battery from a few elements of aluminium alone, sufficient to decompose water and to bring a thin platinum wire to a brilliant glow. The aluminium in this arrangement is of course dissolved. It could, perhaps, be recovered by the means of zinc.—*Liebig's Annalen.* C.

Preservation of India Rubber under Water.—Great losses are often experienced by the users of india rubber tubing, on account of the brittleness which it often acquires in use. A writer in *Dingler's Journal* gives an encouraging account of his success in remedying the difficulty by laying the pipes in water which is often renewed. Even the thickest and stiffest tubes remain soft and pliable, without any perceptible diminution of elasticity, and he has been unable to discover any trace of injurious change. For some uses he soaks the pipes in melted paraffine. When they are kept in water they undergo great changes of color, and upon cut surfaces they often appear greasy and bleached, but all the changes seem beneficial rather than otherwise. Thin rubber bands, however, often become so brittle that they can be easily rubbed into small pieces by the fingers.—*Dingler's Journal.* C.

Hardening Stone.—Dr. Gehring, of Landshut, Bavaria, has invented an enameling liquid which is said to render ordinary stones and cements harder than granite and to facilitate the imitation of marbles and other valuable minerals. When applied to metals he claims that it will be found an excellent preservative against rust.—*Les Mondes.* C.

Regulator of Vapor Pressure.—M. D'Arsonval describes an instrument which he has tested by daily use for more than three years, and which accomplishes the following purposes: 1. To maintain the pressure of any given vapor constant in a boiler whatever may be the supply. 2. To use fuel only in proportion to the quantity of vapor required. 3. To make the action of the instrument completely automatic and without danger of explosion.—*Comptes Rendus.* C.

Improved Electric Batteries.—Azapis has modified the Bunsen cell, using a solution of cyanide of potassium, caustic potash, chloride of sodium or sal ammoniac, instead of dilute sulphuric acid, thus reducing the consumption of zinc and effecting greater constancy of current. Woehler places a roll of sheet aluminum in a round glass vessel containing very dilute hydrochloric acid or dilute caustic soda. Within this larger roll is placed a porous cell, containing concentrated nitric acid, and a smaller roll of aluminum. Each roll has a lug or projection, which is inserted into a circular cover of ebonite and thus kept in place.—*L'Ingen. Univ.* C.

Electric Radiometer.—Bertin has repeated before the French Physical Society the first series of his experiments upon the electric radiometer. When the pressure of the air in the radiometer reaches 130 mm. (5·12 in.) the rotation begins, but it is uncertain; at 90 mm. (3·54 in.) it is decided, but its direction is always determined by some defect of symmetry in the apparatus. At 30 mm. (1·18 in.) it ceases, but may be produced by a spark or warming one of the tubes; it acts as if a wind blew from the cold to the warm pole. At 15 mm. (·59 in.) the electric rotation occurs alone, and is positive, as if the wind came from the positive pole. At 10 mm. (·39 in.) there is no rotation. From 5 mm. to 2 mm. (·197 to ·079 in.) the rotation is negative. From 2 mm. to ·2 there is no rotation. At 1 mm. (·004 in.) and less the rotation is always negative, the negative tube being fluorescent.—*Chron. Industr.*

New Roof.—It is thought that the new roof on the cupola of St. Peter's Cathedral, in Rome, will be finished in two years. It was begun seventeen years ago. The roof is divided into sixteen sections, each of which requires one million pounds of lead.—*L'Ingen. Univ.* C.

Dependence of Electric Conductivity in Coal upon Temperature.—J. Borgmann claims some priority in investigations of the electric conductivity of coal, and reports a series of experiments by which an increase of conductivity at increased temperature was shown in every instance. He found, also, that the light rays produced a greater effect than the dark rays.—*Ann. der Phys. und Chim.* C.

Poteline.—M. Potel has communicated to the French Société d'Encouragement a compound which may be employed in hermetically sealing bottles and flasks, or in making an artificial marble for the ready manufacture of various useful and ornamental articles. It is composed of gelatine, glycerine and tannin; sulphate of baryta or zinc white may be added to make it opaque, and it may be dyed of any desirable tint by means of ordinary vegetable colors.—*Chron. Industr.* C.

Remedy for London Smoke.—W. D. Scott-Moncrieff proposes to extract one-third of the gas from the coal before it is used in London fires. This would require that three times the quantity should be passed through the retorts, but the advantages would be very great. The companies would double the quantity of by-product, in the shape of tar and ammoniacal liquids; the community would have 24-candle gas instead of 16-candle gas; the fuel resulting from the process would light readily, and it would make a cheerful fire, giving out 20 per cent. more heat than common coal; and London would become a smokeless city. The smokeless fuel, which results from an extraction of 3333 cubic feet of gas per ton, has a heating capacity fully 20 per cent. greater than common coal, and 10 per cent. greater than coke. He calculates that there will be an annual balance in favor of his scheme of £2,125,000. This may be taken as the yearly value of London smoke, which is now wasted, but which he proposes to convert into useful products by the plant that is now in use.—*L'Ingen. Univ.* C.

Satellite Geology.—In presenting the Annual of the Bureau of Longitudes, M. Faye described, as usual, the improvements which have been introduced into the volume. The one which will probably attract the most general attention for its novelty is a notice by M. Faye himself upon the comparative geology of the Moon and the Earth, with illustrative plates.—*Comptes Rendus.* C.

American Vines in Europe.—In a report upon Vimont's memoir on the phylloxera, M. Zundel speaks highly of the success which has attended the introduction of American vines. The effectual resistance of certain varieties to insect attacks has been thoroughly established when they are placed in suitable soils. Some of the vines produce good wines, of fine color and rich in alcohol from their own native fruit. The French varieties, such as the Aramon and Chasselas, yield abundantly when grafted upon American stocks, and the quality of the grape is not injured in any way by the grafting.—*Bull. de la Soc. de Mulhouse.* C.

Electric Expansion.—G. Quincke gives a summary of the observations which have been made upon the expansion of Leyden jars and other glass vessels, under the influence of electricity, from the days of Fontana to the present time. He has communicated to the Berlin Academy the results of some of his own investigations, in which he considers the various influences of positive and negative electricity, the resistance of the discharger, the intensity of the electric current in the glass, the thickness and condition of the wall of the thermometer bulb, the fluid in the thermometer, the elasticity of the glass, and various other modifying circumstances.—*Wiedemann's Annalen.* C.

Zincography.—M. Thonrocq, a Paris lithographer, has been very successful in substituting zinc for lithographic stones. By using 5000 zinc matrices, worth 38,500 francs ($7700), he has avoided an expenditure of 250,000 francs ($50,000) for stones, besides considerable saving in the cost of handling and manipulating. He has published in this way more than 350,000 copies for some of the leading French editors, the Polytechnic School, the Department of Bridges and Highways, the Ministry of Public Works and different municipalities. Each plate is good for 10,000 impressions, and the change seems desirable in all cases where large editions are to be worked off within a brief period.—*Bull. de la Soc. d'Encour.* C.

Brest Diving-Bell.—H. Hersent describes a diving-bell which was employed by himself and M. Castor for removing the rocks in the harbor of Brest. The rocks were hard and massive, consisting of schists mixed with quartz, granite, etc., and the work was accomplished safely, rapidly and with great economy. The bell is so arranged as to be easily used for all kinds of submarine work.—*Mem. de la Soc. des Ingen. Civ.* C.

Smoke and Steam under the Microscope.—L. J. Bodaszewsky calls attention to the rapid oscillatory movements which are disclosed by the microscope in the smoke of burning paper, wood, cigars, etc., when concentrated sun or electric light is thrown upon them through a lens. The particles are of a spherical form, and they are continually darting against each other, so as to represent very strikingly the motion of gas molecules according to the kinetic theory. Similar movements are observed in the vapors of nitric, sulphuric and phosphoric acids, sulphur, ammonia, etc., when examined under the microscope by the light of a glowing platinum wire.—*Dingler's Journal.* C.

Removal of a Furnace "Bear."—A furnace near Vienna, which has been out of blast for four years, was obstructed by "bear," or "sow," that is, a mass of slag and iron, which resisted all attempts at removal by breaking or wedging. The mass consisted of two cylinders, the lower one from 16 in. to 80 in. high and 13 ft. 4 in. in diameter, and the upper one between 8 ft. and 9 ft. in both dimensions. The latter was a mixture of slag and iron, while the former consisted of nearly pure metal. Six holes were bored in the junction of the cylinders to an average depth of 32 in. They were loaded with gelatine dynamite and fired by electricity. By using 27 charges, amounting in all to 7·7 lbs. of dynamite, the two masses were separated. To divide the lower mass, 19 bore holes were used, employing in all 89 charges and 20 lbs. of dynamite. The division of the larger fragments required 22 lbs. of dynamite, 32 detonators and four coils of safety fuse. About 67½ tons of good metal were recovered, at a total cost of £30. The shaking of the furnace masonry by the explosions was very slight, and no sensible damage was produced.—*L'Ingen. Univ.* C.

Vanadium Ink.—Berzelius found that by treating an infusion of galls by a solution of vanadate of ammonia, in place of sulphate of iron, he could produce an ink of remarkably good quality. At the time of his discovery, in 1831, it was of no practical interest, because the vanadates were very costly. At the present time their cost has been so much reduced that his recipe can be employed for ordinary inks, which have the additional advantage of presenting great resistance to most reagents and destructive materials. Gum arabic can be dispensed with, and the chance of moulding or alteration thus reduced. —*Chron. Industr.* C.

Absorption Spectrum of Ozone.—J. Chappuis has continued his experiments upon ozone, and finds that the absorption spectrum of oxygen, which has been ozonized by electricity, presents eleven obscure bands in the part of the spectrum which is ordinarily visible. One of the bands is in the orange, and corresponds with the telluric bands which Angström noticed and attributed to something else than water vapor. Two of the other bands of Angström appear also to be due to ozone. The relative stability of ozone at low temperature and low pressure, and its continual production by electric discharges, make it an important element in the upper regions of the atmosphere; its blue color must therefore undoubtedly play an important part in the coloration of the sky.—*Comptes Rendus.* C.

Variations of Luminous Sensibility.—A. Charpentier has experimented with a number of small diaphragms in order to cut off the light from his lanterns and produce images upon the retina having diameters varying between one-sixteenth of a millimetre and a millimetre ($\frac{1}{400}$ in. and ·039 in.). As long as the diameter of the object was superior to two millimetres and the image on the retina was greater than ·176 mm. (·007 in.) no change was noticed in the degree of light required; but below these limits he invariably found, for six different surfaces, that the intensity of light required varied inversely as the luminous surface, so that the product of light by surface was nearly constant. These experiments have already suggested to the investigator numerous interesting questions in regard to the comparative sensitiveness of different portions of the retina and the number of optical cones that are required in order to produce the sensation of light.—*Comptes Rendus.* C.

Length of Jupiter's Day.—The Emperor of Brazil has transmitted to the French Academy a note of M. Crul's upon the time of Jupiter's rotation. The sharpness of outline and the bright color of the brown spot which has been so long visible enabled him to deduce from nearly 1100 rotations a period of 9 h. 55 m. 36 s.—*Comptes Rendus.* C.

Optical Telegraphy.—A. Crova describes an apparatus for sending light signals, which he invented in conjunction with Le Verrier, in 1870, and which he regards as substantially identical with Mercadier's, the principal difference being that he used oil instead of petroleum. The signals were visible in broad daylight, and when the sky was somewhat clouded correspondence could be carried on by day as well as by night. He found it necessary to employ oxygen under a feeble pressure, and to have the manipulating key armed with a strong string, so that it could be suddenly opened and closed. Without this precaution the signal would overlap and produce confusion.—*Comptes Rendus.* C.

Book Notices.

A Physical Treatise on Electricity and Magnetism. By J. E. H. Gordon, B. A. Camb., Assistant Secretary of the British Association. 2 vols., 8vo. New York: D. Appleton & Co. 1880.

The object of the work, as stated by the author, is to give a complete account of certain portions of electrical science, viewed, almost entirely, from a physical rather than a mathematical standpoint. To carry out this idea no mathematical considerations have been introduced into the body of the text that require, on the part of the reader, a more extended knowledge than algebra as far as simple equations. In all cases where a thorough understanding of the subject requires the aid of higher mathematics, explanations are either entirely omitted, or are introduced as foot notes or appendices.

The work is divided into four parts. Part I, 141 pp., treats of Electro-Statics; Part II, 56 pp., of Magnetism; Part III, 323 pp., of Electro-Kinetics, and Part IV, the remainder of the work, of Electro-Optics.

In Part I we find, after the usual preliminary statements regarding

the properties of an electrified body, a short account of electrical machines, an explanation of electric force and potential, and a full description of electrometers, followed by the theory of absolute measurements. The balance of Part I, comprising about two-thirds of the whole, is devoted to the consideration of the determination of specific inductive capacity.

Part II discusses the properties of magnets; explains fully the action of terrestrial magnetism on the needle, treats of the variations of the needle; gives full descriptions of the most recent methods for terrestrial magnetic observations, and concludes with general observations on terrestrial magnetism.

Part III describes briefly the various forms of voltaic batteries; discusses the actions of currents on magnets and on other currents, and defines the electro-magnetic units It also gives full descriptions of the methods employed for measuring electric resistance; discusses electro-magnetism; barely mentions the telephone and Hughes' induction balance; gives a full account of the British Association unit of resistance, and concludes, in Vol. I, with descriptions of the induced currents caused by making and breaking the circuit.

In Vol. II, Part III discusses dia-magnetism; the induction coil and its discharge; the striæ observed in partial vacua. Very full descriptions are given of Spottiswoode and Moulton's researches on the sensitive state of discharges through rarified gases, together with a less extended description of Crooke's investigation of electrical discharges in high vacua. Electrolysis is alloted but eight pages, and a bare mention made of magneto-electric and electro-magnetic engines and the electric light. Part III then concludes with a discussion of thermo-electricity, electricity of contact and electro-magnetic units.

Part IV gives full descriptions of the influence of magnetism in rotating the plane of polarization of light, and discusses the relations between statical electricity and polarized light. The final chapter gives a description of Clerk Maxwell's electro-magnetic theory of light.

In some respects, the book forms a valuable contribution to electrical literature. This is especially the case with Chaps. VII and IX, Vol. I, which treat of electrometers. Here we find a fair compilation which gives an accurate description of the various patterns of electrometers as now made and used. These descriptions are terse and clear, and are illustrated by unusually fine wood cuts. The chapter on the

theory of absolute measurement is excellent, and cannot fail to be of great value to the general student. Specific inductive capacity is ably and fully discussed, and the description of galvanometers is excellent. Many of the topics discussed in Vol. II are well handled.

There is, however, an unfortunate absence of logical connection between many of the different parts of the book that unpleasantly suggest to the reader a want of completeness, a lack of unity of plan. It would seem as though much of the compiled part of the book had either been hurriedly thrown together, or thoughtlessly selected from materials within easy access; while in many cases the written portions appear to have been prepared inconsecutively. The author, too, has given undue prominence to certain topics, and suppressed others connected therewith, in an exceedingly arbitrary manner. As an instance, we find that while the comparatively unimportant subject of the sensitive state of discharges through rarefied gases is assigned some 24 pages, the exceedingly important subject of electric motors, dynamo-electric machines and electric lighting is summarily dismissed after but 9 pages.

It is to be regretted that, in so extended a work on electricity, the author's teachings should be so weak as to the nature of the electrical force. At the very outset of the work, in the first paragraph, we meet the mysterious statement that "We have no conception of electricity apart from the electrified body; we have no experience of its independent existence," a statement which, of course, is true of any phase of energy. Then again, on pages 14 and 15 we read, "For purposes of calculation electricity of either kind may be treated precisely as if it was a material incompressible fluid," and, "For purposes of calculation, any increase or decrease of electrification may be considered to be produced by the addition or by the taking away of a quantity of something (which we call electricity)." How much better it would be for the mind of the reader, which is assumed to be but little trained to mathematical conceptions, if the author stated plainly that the thing added or subtracted was energy. The absence of such a statement is all the more marked, since on page 24, Vol. I, we are given the value of the unit of electromotive force, the volt, in absolute units of potential; while in the closing paragraph of the book, page 276, Vol. II, we read as a conclusion drawn from a brief consideration of Maxwell's "Electro-Magnetic Theory of Light," as follows, viz.: "The

numerous direct relations which exist between light and electricity leave us but little doubt that they are very closely related, and that their effects are but two forms of that common energy whose nature is unknown, but which certainly underlies all physical phenomena."

The publishers are to be complimented on the excellent character of the numerous wood cuts. We regret that we cannot equally compliment them on the typography, since, throughout both volumes, wrong fonts and broken letters are of very common occurrence. E. J. H.

FOUR LECTURES ON ELECTROSTATIC INDUCTION. By J. E. H. Gordon, B.A., Assistant Secretary of the British Association. 8vo. New York: D. Van Nostrand. 1881.

These lectures were delivered at the Royal Institution of Great Britain, in January and February, 1879.

After explaining the general nature of induction, and pointing out the difference between it and conduction, the author discusses the general action of the Leyden jar, and gives a brief account of the theory of lateral pressure accompanying the state of strain produced by induction. The variations in the specific inductive capacities of different substances are then explained, and some account given of the earlier, as also of the more recent determinations of specific inductive capacity. The last lecture contains, among other subjects, a discussion of Clerk Maxwell's electro-magnetic theory of light.

The topical arrangement is good, and the subject matter is, on the whole, well handled, but the work shows either undue haste, or else a lack of judgment in the matter of expression. Instances of the latter occur in passages like the following, viz.: "The glass or sealing-wax, after being rubbed, is found to be in the state called '*electrification.*'" Also, "For every *bit* of positive electricity we produce an exactly equal quantity of negative."

The author leaves his reader in too much doubt as to the nature of electricity. Instead of regarding it, as is now almost universally done, as a phase of energy, and clearly pointing out how every other variety of energy can readily be converted into electric energy, we find such ambiguous teachings as these, viz.: "We do not know whether the properties of an electrified body are caused by one or two electric fluids entering or leaving it, as water into a sponge; or by a motion of

its molecules, as when a body is heated; or by a strain or twist of its structure, as when steel is magnetized." * * * "We have no experience of its independent existence."

Later in the book we find the following remarkable ideas advanced, viz.: "We cannot make or destroy electricity—" * * * "When we rubbed glass, we produced positive electricity on its surface. Was not that a creation of electricity? No; for an exactly equal quantity of negative electricity was produced in the rubber." Here the author is decidedly at fault; we can both make and destroy electricity; that is, we can convert every variety of energy into other forms of energy; and so also we can convert electrical energy into other forms of energy. The argument used by the author to prove his assertion, that electricity was not created, because an equal amount of opposite electricity was produced, leads us to infer that he has the fluid hypothesis in mind when using the illustration. His idea appears to be that nothing has been created; the positive and negative fluids existed previously; the mechanical action of rubbing has simply separated the two fluids; therefore there has been no creation, but merely a separation.

He also makes the following remarkable assertion, "If this were possible (the production of a single kind of electricity without the production of its opposite kind, which of course is impossible), we might actually increase the quantity of electricity in the world. Experiment shows us that we cannot do this." Here again the meaning is ambiguous. If electricity be a species or phase of energy, he is at fault, since nearly all the energy in the universe might be converted into electrical energy, in which case the quantity of electricity in the world would be largely increased. If, however, he regards electricity as a kind of matter, and here we see indications of a lingering belief in a fluid hypothesis, he is right: since of course we can neither increase nor decrease the quantity of matter in the universe. Does the author believe that electricity is a kind of matter? E. J. H.

A TEXT BOOK OF ELEMENTARY MECHANICS for the use of Colleges and Schools. By Edward S. Dana, Assistant Professor of Natural Philosophy in Yale College. 12mo. New York: John Wiley & Sons. 1881.

"For we have done those things which we ought not to have done,

and we have left undone those things which we ought to have done," applies to many things other than our moral transgressions.

Certainly, if an elementary mechanics should have any one virtue, that virtue should be clearness of conception and accuracy of expression.

Glancing through the book, on page 220 we find the following statement: "A toothed wheel is a circular disc provided with teeth on the circumference; such a wheel, turning on one axis, interlocks with a second, turning on another axis, and in this way the *force* applied at the first is communicated to the second. There may be a *mechanical advantage* with a corresponding loss of speed, or a gain of velocity and a consequent *mechanical disadvantage*."

Would it not have been much clearer to have said the *work* applied at the first is communicated to the second there may be a *gain in force* with a corresponding loss of speed, or a gain in velocity and a consequent *loss in force*.

"A mechanical advantage" may be gained from a given quantity of work in either direction; that is, you may gain greater speed or greater force at will, and a loss in force may prove anything but a mechanical disadvantage in many cases.

The use of the word "power" (see page 222) in the sense of force is also confusing. The power of a machine is the number of foot-pounds of work it will do in the unit of time taken. Prof. Dana is not alone in this erroneous use of language, but takes this word from others, who make far greater pretensions as mechanicians than he does in his modest preface.

Although we have four chapters on dynamics or kinetics, it is impossible to find in them any reference to the moment of inertia, or to the radius of gyration. There is the trouble with many af our elementary mechanics: the students read them, and it is true, too, that the great majority forget what they have read as rapidly as possible afterward; but they have also gained the false impression that they at least know the names of the fundamental conceptions of mechanics.

All who have in after life been obliged to work beyond the elements of any science know how very hard it is to get rid of youthful impressions and modes of thinking; it is, like speech, almost an incorrigible fault if acquired from an incorrect model.

Quoting from the preface, "The study of elementary mechanics is one of very great value in a course of liberal education;" but it had a great deal better be left out altogether than be wrongly taught.

In many cases it can at once be perceived that the writer has used a work on elementary mechanics as a thread on which to string hundreds of ingenious and elegant but *utterly useless* mathematical problems —useless to the mechanic we mean, not to the mathematician.

The amount of mathematics which can be used in an elementary mechanics, adapted to the capacity of the average collegian, is extremely small, and they must, too, be of the simplest kind. Why do not our writers endeavor to give, in intelligible language, a clear conception of what the fundamental ideas of mechanics are? Those who take an interest in the study will naturally have more than the average of mathematical acquirements in after life, and will be in a position to start from clearly defined ideas and thoroughly understood premises.

We are told that "momentum is equal to the product of the mass and velocity," and, in other words, that "the moment of inertia is equal to the mass multiplied by the square of the radius of gyration." What more do these statements convey to the mind of the student than the statement that two times two is four? Why not say that the momentum is equivalent to that constant force which, acting for one second, will bring the moving body to rest, and that the moment of inertia is equivalent to the statical moment of the sum of the momentums of all the particles of a revolving body? We then have at least some grasp of the subject.

We regret also to see that the part devoted to machines has been divided up into seven parts, each devoted to one of the so-called elementary machines. The lever, the cord and the inclined plane would have been all sufficient and covered every case. For instance, the screw is but a modification of the inclined plane, and although six pages are devoted to its consideration, we are nowhere informed of what is known to every machinist, that square-threaded screws must be used when the screw is intended to impart motion to its nut, and that V-threaded screws are used for the purpose of clamping two bodies together.

The reviewer does not wish to give the impression that Prof. Dana's little work is any more in error than the books from which he has compiled his manual, but has seized this opportunity to enter a protest against the writing of works which mislead by inaccurate language and by causing the student to believe that he has at least had a glimpse of the whole field of mechanics. W. D. M.

INTERMEDIATE LESSONS IN NATURAL PHILOSOPHY. By Edwin J. Houston, A.M., Professor of Physical Geography and Natural Philosophy, Central High School of Philadelphia, etc. 12mo. Philadelphia: Eldredge & Brother. 1881.

This volume is the latest addition to the very useful series of this author's school books. As announced in the preface, it is intended to fill the gap between his "Easy Lessons in Natural Philosophy" and his "Elements of Natural Philosophy." Prof. Houston's long experience as a teacher stands him in good stead as an author. He has no difficulty in placing himself *en rapport* with his pupils. He appreciates their difficulties and anticipates them. The work here noticed is characterized by the same clearness of explonations that we have had occasion to praise in noticing the author's earlier volumes; but while the text of his book is lucid and adapted for the comprenension of partly advanced students, it is at the same time strictly accurate.

The book is liberally illustrated, many of the engravings being new to works of this grade; some of them have been specially prepared for the work, while others are excellent reproductions from more elaborate French and German works. By lending attractiveness to the subject they add materially to the educational value of the book.

The publishers have issued the work in very creditable style; it is well printed and substantially bound, and has the look of serviceability that a school book should have. W. H. W.

Franklin Institute.

HALL OF THE INSTITUTE, April 20th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 129 members and 43 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced, that at their last meeting 15 persons were elected members of the Institute; he also read a preamble and resolutions with regard to the creation of a Trust for a Building Fund for the Institute, offered by the Committee on Stocks and Finance, and adopted by the Board of Managers at their meeting on April 13th, 1881.

Mr. William Sellers, Mr. Frederick Fraley and Mr. J. Vaughan Merrick were appointed the Trustees by the Board.

The Secretary reported the following donations to the Library:

Journal Franklin Institute. 112 numbers. 1870 to 1881.
From J. E. Mitchell.

Text-Book of Elementary Mechanics. By E. S. Dana..
From John Wiley & Sons, Publishers, New York.

American Sanitary Engineering. By E. S. Philbrick.
From Publisher of *Sanitary Engineer*, New York.

Illustrated Catalogue of Locomotives.
From J. B. Lippincott & Co., Publishers.

Forty-seventh Annual Report of the Royal Cornwall Polytechnic Society. Falmouth. 1879. From the Society.

Aeneidea, or Critical Remarks, etc., on the Aeneis. By Jas. Henry. Vol. 2. Dublin. 1879.

Memoirs of the Geological Survey of India. Vol. 15, Part 2; Vol. 17, Parts 1 and 2 and Record.

Palœontologia Indica. Ser. X, Parts 1 and 2, Vol. 1; Ser. XIII, Parts 1 and 2. Salt Range. From the Geological Survey Office.

Revista Cientifica Mexicana. Vol. 1, Nos. 10 to 12. 1880.
From the Publisher.

Transactions of the Royal Irish Academy. I. M. Series. Vol. 1. Science Series, Vol. 26; "Cunningham Memoirs." No. 1 and Proceedings. From the Society.

Annual Report of the State Geologist of New Jersey for 1880.
From G. H. Cook, S. G.

Early Society in Southern Illinois. By R. W. Patterson. And History of the Chicago Historical Society. From the Society.

The New Improved Bed of the Danube at Vienna. By Sir G. Wex. Washington. 1881.

Index to the Library of the American Society of Civil Engineers. Part 1, Railroads. From the Society.

Memoir of J. A. Meigs, M.D. By L. Turnbull, M.D. Philadelphia. 1881. From the Author.

Endowment of Scientific Research. By G. Davidson.

Variable Stars of Short Period. By E. C. Pickering. Cambridge. 1881. From the Author.

Opinion delivered by Mr. Justice Bradley in the case of R. A. Tilghman *vs.* Wm. Proctor. From Geo. Harding.

Meteorological Researches for the use of the Coast Pilot. Part 2. On Cyclones. From the U.S. C. & G. Survey.

Milling World and Chronicle of the Grain and Flour Trade. Buffalo, N. Y. From the Publisher.

Nouvelles Annales de la Construction. 1861—1863; 1865—1868; 1870—1874; and 29 unbound pamphlets. Also, Catalogue of Centennial Exhibition. 1876. From Lewis S. Ware.

Second Annual Report of the Indiana Bureau of Statistics and Geology. 1880. From the Bureau.

Annual Report of the Board of Regents of the Smithsonian Institution for 1879. From the Institution.

Alphabetical Lists of Patentees, etc. July—Dec., 1880. From the U. S. Patent Office.

Manual of Engineering and Mathematical Instruments made by Young & Sons. From Young & Sons.

Annual Reports of the Massachusetts Board of Education. 1850—1878–79 inclusive. From the Board, Boston.

Reports of the Second Geological Survey of Pennsylvania. G^4; H^5; I^3; P. R. From the Board.

Total Solar Eclipses of July 29. 1878. Appendix III to Observations for 1876. From the U. S. Naval Observatory.

Report of the Superintendent of U. S. Coast Survey for 1877. From the Coast Survey Office, Washington.

Annual Report of the Secretary of the Treasury on the State of Finances for 1880. From the Treasury Dept., Washington.

Annual Report of the Commissioner of Education for 1878. From the Bureau.

Annual Report of the Chief of Ordnance for 1880. From the Chief of Ordnance.

Manchester Steam User's Association Chief Engineers' Annual Refort for 1879. From the Association.

Steel Compressing Arrangements at the Barrow Works. By A. Davis. From the Author, London.

Report of the Department of Public Works, City of New York, for 1880. From the Department.

Report of the Commissioner of Internal Revenue for 1880. From the Commissioner.

Transactions of the American Philosophical Society. Vol. 15 N. S. Part 3. From the Society.

The Virginias, a Mining, Industrial and Scientific Journal. Edited by Jed. Hotchkiss. Vol. 1, 1880.
From W. C. Macdowell, Philadelphia.

Memorial of Joseph Henry. Washington. 1880.
From Hon. Chas. O'Neill, M. C.

Annual Report of the Adjutant-General of Pennsylvania for 1880.
From the Adjutant-General.

Smull's Legislative Hand-Book for 1881.
From G. W. Hall.

The Millstone. Vols. 2, 3 and 4. From the Publishers.

Houston's Intermediate Lessons in Natural Philosophy. Philadelphia: Eldredge Bros. 1881. From the Author.

Four Lectures on Static Electric Induction. By J. E. H. Gordon. New York. 1881. From D. Van Nostrand, Publisher.

Mr. J. Snowden Bell read an interesting paper on the Wootten Locomotive Engine, illustrating his remarks with drawings and diagrams. The paper is printed in full on page 340 of the JOURNAL.

Dr. Grimshaw inquired whether any experiments had been made with large drivers such as are used on English and French railways for fast service. He believed that the drivers on the Wootten engines were only about five and a-half feet in diameter, but on English railways they were made from nine feet to three metres in diameter.

Mr. Bell in reply said that the largest drivers he had ever heard of as being used in this country were eight feet in diameter. They were used on engines built thirty years ago. The largest now in use, he believed to be those on the new engine of the Pennsylvania road and they are only six feet six inches. It is certainly desirable to have large drivers, but in practical construction builders are limited by the requirements for the fire box.

Mr. Nystrom said that the diagrams showed the boiler elevated high above the track, this being made necessary by putting it over the driving wheels. He thought it would be better to turn the boiler end for end, bringing it down over the smaller or truck wheels.

Mr. Bell said that the great elevation of the boiler of the Wootten locomotive was invariably the first subject of criticism, but in practical work it did not prove objectionable. The Italian engineers found upon trial that the centre of gravity was not inordinately high. When he (Mr. Bell) worked in a locomotive engine workshop some years ago, it used to be the rule to put the boilers as low as possible. Now, the greater elevation is not objected to and some engineers hold it to be, in fact, an advantage.

Mr. Shaw said that he also had an impression upon looking at the Wootten locomotive that it was too high. He did not like the idea of riding on it, but he soon acquired confidence in the engine after trial. He commented on Mr. Nystrom's suggestion about turning the boiler around under the misapprehension that that gentleman proposed to turn the engine itself around, making the drivers run first.

Mr. Nystrom explained that he simply meant to move the boiler end for end, and in answer to a running fire of questions said he thought the centre of gravity would come all right, that the weight would be on the drivers, and finally, when asked what he would do with the cylinders, said he would arrange for them when he came to make the plan.

The Secretary's report included Ericsson's new caloric pumping engine which was exhibited by the H. B. Smith Machine Company. It is a very simple caloric engine, adapted for use in private residences, hotels, etc., and to be operated by unskilled labor. The air is alternately expanded and contracted in the chamber without allowing any of it to escape. This is accomplished by the use of a large displacing piston fitting loosely into the cylinder, and used to drive the air from end to end, without allowing any of it to escape, thus doing away with all valves. The air is first brought into contact with the lower part of the cylinder, where it is heated and expanded by a gas or coal furnace, and then carried to the upper part, where it is cooled by the water that is being pumped, and which passes directly from the pump into an annular space or jacket around the upper end of the cylinder, and thence to the discharge pipe. An air-tight piston closes the upper end of the air cylinder, and, when lifted by the expanding air beneath

moves the walking beam, and lifts the pump rod. The heated air upon being driven to the cold end of the cylinder is instantly cooled and contracted, and the momentum of the fly-wheel carrying the air-tight piston down, lifts the displacer, and thus drives the air to the hot end, when the operation is repeated. The air may be expanded and contracted three hundred times per minute. Either coal or gas can be used for heating, the latter being preferable, as the consumption is only fifteen cubic feet per hour. The engines are made of various sizes, from the six inch, capable of lifting 200 gallons of water fifty feet per hour, to the duplex 12-inch, capable of lifting 1600 gallons fifty feet per hour. The machine is entirely safe from dangers of explosion, and is very simple in its operation, and not likely to get out of order. The engine was used to pump water from a tub through a curved pipe and back again to the tub.

Mr. Holman called attention to the fact that the pipe was acting as a siphon and that the water was not being pumped the full height of the pipe.

The exhibitor said that was no doubt true, but the pump was capable of lifting 200 gallons per hour to a height of fifty feet. Having said that the engine was best adapted for pumping purposes because the water was needed to cool the cylinder, but that the engine might be used as a motor if the city water was used for cooling the cylinder, he was asked by Dr. Grimshaw whether water under the same head as that thus used if employed to drive a turbine would not yield as good results as the engine. He replied that that might be so; he had not examined the question. It was not, however, intended that these engines should be used as motors but only for pumping purposes.

Breitinger & Kunz exhibited Wenzell's system of air clocks, which has been in successful use in California for five years. The main clock or regulator must, of course, be a good time keeper. It is connected with a simple air pump worked by wheel work, and from the pump or pumps a small gas pipe is led away to the dial works, forty or more in number. These works are extremely simple, their only function being to provide for the movement of the hands in response to the pulsations of the air pumped by the regulator. The dials may be made of almost any desired form and cased entirely dust proof.

Dr. Grimshaw inquired as to the relative cost of the leaden

pipes used with these clocks and the copper wire used for electrical clocks.

Mr. Breitinger, the exhibitor, answered that that was a subject he had not considered and that he did not think it necessary to consider it, as the pipes need not be of lead Gas pipes would serve the purpose. India rubber tubes were only used at the Institute for convenience.

Prof. Marks said that Prof. Rogers had experimented with a similar clock, and he understood had finally abandoned it because it was subject to so many accidents from variations in the state of the atmosphere.

Mr. Breitinger called attention to the fact that this apparatus was not open to such objections as the air was set free at each movement, the plunger or pump piston being lifted clear of the solution after each stroke.

Mr. Shaw also said that he did not think Prof. Marks would find this similar to the pneumatic pumps with which he had experimented. The pumps here used are miniature gasometers operating at a distance another gasometer. He inquired how far they would work, and Mr. Breitinger replied, from 1000 to 1200 feet with certainty. The liquid into which the piston is plunged is glycerin.

Mr. Shaw said he thought it a very worthy apparatus.

In answer to Mr. Spellier, Mr. Breitinger said that the heavy weights were required to drive the pumps. They are needed to run four dials, but would run forty just as well.

The United States automatic gas machine is an ingenious combination of devices for the generation of gas from the lighter oils and combining it with air for lighting and heating purposes. It is entirely automatic and, having been put in operation, makes the gas just as it is needed for consumption, no supply being kept on hand. The gasoline according to the plan is to be put in the yard in a cask connected by a small lead pipe with a pump in the cellar on the same level. There are also two cylinders, one to be heated by the gas of the machine, into which the oil is pumped in small quantities, and in which the gas is generated; the other an air cylinder from which air to be mixed with the gas is pumped by the movement of a piston actuated by the expansion of the gas in the process of its formation. Mechanical devices are employed to regulate the time of pumping, and thus to regulate automatically the supply of gas for

consumption. The cylinder in which the gas is generated is surrounded by water, heated from 200 to 212 degrees by a small gas burner, using the gas that it generates.

Mr. E. C. Burgess said that the expense of a 100 light machine in this city, set up ready for use, would be about $400. What is known as a 100 light machine would really run 150 lights, but the lower limit was given in its title so as to be safely within the bounds of truth. The gas is delivered under a two-inch pressure. It requires about six gallons of oil per thousand cubic feet. The oil costs at present about fifteen cents per gallon, making the cost of gas ninety cents per thousand cubic feet. The ordinary price of gasoline is about ten cents, making the cost about sixty cents per thousand.

Mr. Shaw having obtained from the exhibitor a more detailed statement of the method of manufacture said that he had developed before the Institute what he desired to show, that this was really a vapor *engine*, which, after using the vapor to drive the machine then employed it for illuminating purposes. He thought this a novelty worthy of being specially mentioned.

Prof. Marks inquired whether the apparatus had been worked long enough to ascertain whether tar was deposited in the machine or in the burners.

Mr. Burgess said that the machine had been in use for about one year, and there was no sign of any deposit anywhere. The gasoline appeared to be completely vaporized. If there was any residual product it had not been made visible.

The Secretary closed his report by calling attention to one of Ladd's swing chairs, which was on exhibition. This is suspended in an ingenious manner, and the body and back of it can be adjusted to any desired position, and which may be instantly varied with very little effort to the occupant of the chair. As the supporting frames are of steam bent ash, great strength and lightness are obtained, and when not in use it can be folded so as to occupy very little room.

Owing to the lateness of the hour, Mr. McKean moved that when the Institute adjourns it be to meet Wednesday evening, April 27th, and that the first business in order be the Report of the Board of Managers on the subject of obtaining a building fund, which was agreed to.

On motion, the Institute adjourned.

ISAAC NORRIS, M.D., *Secretary.*

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PRODUCTION OF SOUND BY RADIANT ENERGY.

By Alexander Graham Bell.

A paper read before the National Academy of Arts and Sciences April 21, 1881

In a paper read before the American Association for the Advancement of Science last August, I described certain experiments made by Mr. Sumner Tainter and myself which had resulted in the construction of a "Photophone," or apparatus for the production of sound by light;* and it will be my object to-day to describe the progress we have made in the investigation of photophonic phenomena since the date of this communication.

In my Boston paper the discovery was announced that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight. The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of

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* Proceedings of the American Association for the Advancement of Science, Aug. 27, 1880; see also *American Journal of Science*, vol. xx, p. 305; *Journal of the American Electrical Society*, vol. iii, p. 3; *Journal of the Society of Telegraph Engineers and Electricians*, vol. ix, p. 404; *Annales de Chimie et de Physique*, vol. xxi.

the various substances which became sonorous in the condition of thin diaphragms; but this failure was explained upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was therefore supposed that if we could lead to the ear air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. The first experiments made to verify this hypothesis pointed towards success. A beam of sunlight was focussed into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam a clear musical tone was heard, the pitch of which depended upon the frequency of the interruption of the light and the loudness upon the material composing the tube. At this stage our experiments were interrupted, as circumstances called me to Europe.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds produced by masses, but would also permit us to test the more general proposition that *sonorousness, under the influence of intermittent light, is a property common to all matter.*

The substance to be tested was to be placed in the interior of a transparent vessel, made of some material which (like glass) is transparent to light, but practically opaque to sound.

Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing-tube.

Some preliminary experiments were made in Paris to test this idea, and the results were so promising that they were communicated to the French Academy on the 11th of October, 1880, in a note read for me by Mr. Antoine Breguet.* Shortly afterwards I wrote to Mr. Tainter, suggesting that he should carry on the investigation in America, as circumstances prevented me from doing so myself in Europe. As these experiments seem to have formed the common starting-point for a series of independent researches of the most important character carried on simultaneously in America by Mr. Tainter, and in Europe by M.

* *Comptes Rendus*, vol. xci, p. 595.

Mercadier,* Prof. Tyndall,† W. E. Röntgen,‡ and W. H. Preece,§ I may be permitted to quote from my letter to Mr. Tainter the passage describing the experiments referred to:

"Metropolitan Hotel, Rue Cambon, Paris, Nov. 2, 1880.

"Dear Mr. Tainter—* * * * I have devised a method of producing sounds by the action of an intermittent beam of light from substances that cannot be obtained in the shape of thin diaphragms or in the tubular form; indeed, the method is specially adapted to testing the generality of the phenomenon we have discovered, as it can be adapted to solids, liquids and gases.

"Place the substance to be experimented with in a glass test-tube, connect a rubber tube with the mouth of the test-tube, placing the other end of the pipe to the ear. Then focus the intermittent beam upon the substance in the tube. I have tried a large number of substances in this way with great success, although it is extremely difficult to get a glimpse of the sun here, and when it does shine the intensity of the light is not to be compared with that to be obtained in Washington. I got splendid effects from crystals of bichromate of potash, crystals of sulphate of copper, and from tobacco smoke. A whole cigar placed in the test-tube produced a very loud sound. I could not hear anything from plain water, but when the water was discolored with ink a feeble sound was heard. I would suggest that you might repeat these experiments and extend the results," etc.

Upon my return to Washington, in the early part of January,‖ Mr. Tainter communicated to me the results of the experiments he had made in my laboratory during my absence in Europe. He had commenced by examining the sonorous properties of a vast number of substances enclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard, rigid bodies like crystals, or diaphragms such as we had hitherto used.

* "Notes on Radiophony," *Comptes Rendus*, Dec. 6 and 13, 1880; Feb. 21 and 28, 1881. See also *Journal de Physique*, vol. x, p. 53.

† "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." *Proc. Royal Society*, Jan. 13, 1881, vol. xxxi, p. 307. [Reprinted in Journal, p. 297.]

‡ "On the Tones which Arise from the Intermittent Illumination of a Gas." See *Annalen der Phys. und Chemie*, Jan., 1881, No. 1, p. 155.

§ "On the Conversion of Radiant Energy into Sonorous Vibrations." *Proc. Royal Society*, March 10, 1881, vol. xxxi, p. 506.

‖ On the 7th of January.

In order to study the effects under better circumstances he enclosed his materials in a conical cavity in a piece of brass, closed by a flat plate of glass. A brass tube leading into the cavity served for connection with the hearing tube. When this conical cavity was stuffed with worsted or other fibrous materials the sounds produced were much louder than when a test-tube was employed. This form of receiver is shown in Fig. 1.

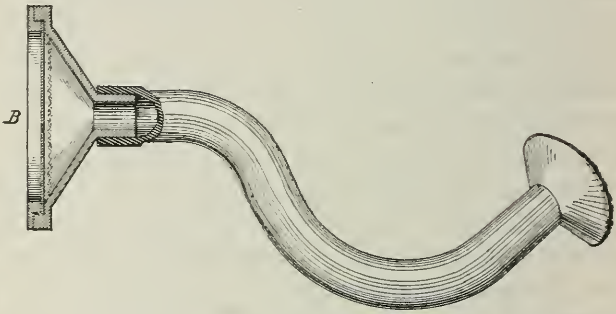

Fig. 1.

Mr. Tainter next collected silks and worsteds of different colors, and speedily found that the darkest shades produced the best effects. Black worsted, especially, gave an extremely loud sound. As white cotton-wool had proved itself equal, if not superior, to any other white fibrous material before tried, he was anxious to obtain colored specimens for comparison. Not having any at hand, however, he tried the effect of darkening some cotton-wool with lamp-black. Such a marked reinforcement of the sound resulted that he was induced to try lamp-black alone. About a teaspoonful of lamp-black was placed in a test-tube and exposed to an intermittent beam of sunlight. The sound produced was much louder than any heard before. Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lamp-black surface towards the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lamp-black surface turned from the sun the sound was much feebler.

Mr. Tainter repeated these experiments for me immediately upon my return to Washington, so that I might verify his results.

Upon smoking the interior of the conical cavity, shown in Fig. 1,

and then exposing it to the intermittent beam, with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube. The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver, as illustrated in the drawing, Fig. 1.

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor reinforcements were constantly occurring, which became more and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The effects produced by lamp-black seemed to me to be very extraordinary, especially as I had a distinct recollection of experiments made in the summer of 1880 with smoked diaphragms, in which no such reinforcement was noticed.

Upon examining the records of our past photophonic experiments we found in vol. vii, p. 57, the following note:

"Experiment V.—Mica diaphragm covered with lamp-black on side *exposed* to light.

"Result: Distinct sound about same as without lamp-black.—*A. G. B.*, July 18th, 1880.

"Verified the above, but think it somewhat louder than when used without lamp-black."—*S. T.*, July 18th, 1880.

Upon repeating this old experiment we arrived at the same result as that noted. Little, if any, augmentation of sound resulted from smoking the mica. In this experiment the effect was observed by placing the mica diaphragm against the ear, and also by listening through a hearing-tube, one end of which was closed by the diaphragm. The sound was found to be more audible through the free air when the ear was placed as near to the lamp-black surface as it could be brought without shading it.

At the time of my communication to the American Association I

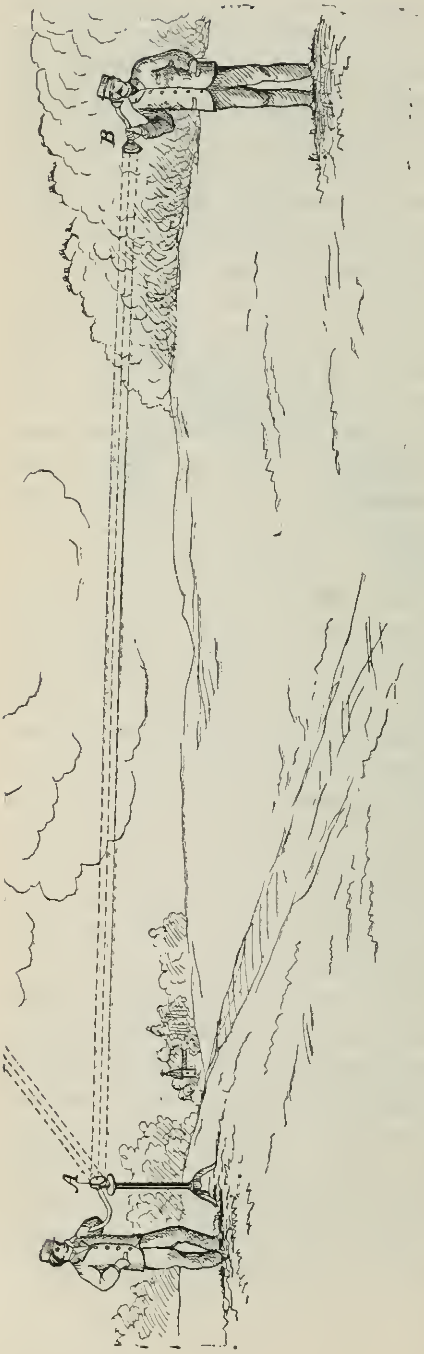

Fig. 2

had been unable to satisfy myself that the substances which had become sonorous under the direct influence of intermittent sunlight were capable of reproducing the sounds of articulate speech under the action of an undulatory beam from our photophonic transmitter. The difficulty in ascertaining this will be understood by considering that the sounds emitted by thin diaphragms and tubes were so feeble that it was impracticable to produce audible effects from substances in these conditions at any considerable distance away from the transmitter; but it was equally impossible to judge of the effects produced by our articulate transmitter at a short distance away because the speaker's voice was directly audible through the air. The extremely loud sounds produced from lampblack have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed.

The drawing (Fig. 2) illustrates the mode in which the experiment was conducted. The diaphragm of the transmitter (A) was only 5

centimetres in diameter, the diameter of the receiver (B) was also 5 centimetres, and the distance between the two was 40 metres, or 800 times the diameter of the transmitting diaphragm. We were unable to experiment at greater distances without a heliostat on account of the difficulty of keeping the light steadily directed on the receiver. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lamp-black receiver.

In Fig. 3 is shown a mode of interrupting a beam of sunlight for producing distant effects without the use of lenses. Two similarly perforated disks are employed, one of which is set in rapid rotation, while the other remains stationary. This form of interrupter is also admirably adapted for work with artificial light. The receiver illustrated in the drawing consists of a parabolic reflector, in the focus of which is placed a glass vessel (A) containing lamp-black or other sensitive substance, and connected with a hearing-tube. The beam of light is interrupted by its passage through the two slotted disks shown at B, and in operating the instrument musical signals like the dots and dashes of the Morse alphabet are produced from the sensitive receiver (A) by slight motions of the mirror (C) about its axis (D).

In place of the parabolic reflector shown in the figure a conical reflector like that recommended by Prof. Sylvanus Thompson * can be used, in which case a cylindrical glass vessel would be preferable to the flask (A) shown in the figure.

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the color are two conditions that markedly influence the intensity of the sonorous effects. *The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colors.*

The materials from which the best effects have been produced are cotton-wool, worsted, fibrous materials generally, cork, sponge, platinum, and other metals in a spongy condition, and lamp-black.

The loud sounds produced from such substances may perhaps be explained in the following manner: Let us consider, for example, the case of lamp-black, a substance which becomes heated by exposure to rays of all refrangibility. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass the particles of lamp-black are

* *Phil. Mag.*, April, 1881, vol. xi, p. 286.

heated, and consequently expand, causing a contraction of the air-spaces or pores among them.

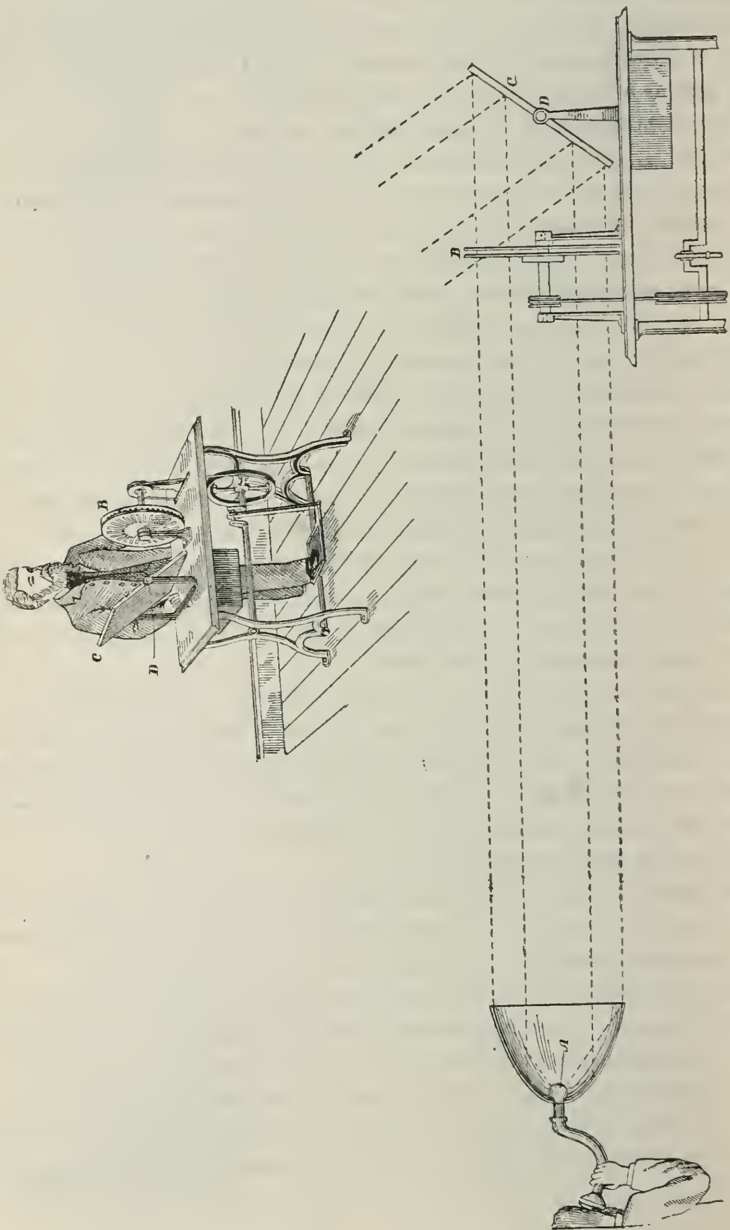

Fig. 3.

Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge.

The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lamp-black. When the light is cut off the converse process takes place. The lamp-black particles cool and contract, thus enlarging the air-spaces among them, and the enclosed air also becomes cool. Under these circumstances a partial vacuum should be formed among the particles, and the outside air would then be absorbed, as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon lamp-black, and a wave of rarefaction is originated when the light is cut off. *We can thus understand how it is that a substance like lamp-black produces intense sonorous vibrations in the surrounding air, while, at the same time, it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.*

This curious fact was independently observed in England by Mr. Preece, and it led him to question whether, in our experiments with thin diaphragms, the sound heard was due to the vibration of the disk or (as Prof. Hughes had suggested) to the expansion and contraction of the air in contact with the disk confined in the cavity behind the diaphragm. In his paper, read before the Royal Society on the 10th of March, Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to the vibrations of the confined air, and that the *disks do not vibrate at all.* I shall briefly state my reasons for disagreeing with him in this conclusion:

1. When an intermittent beam of sunlight is focussed upon a sheet of hard rubber or other material a musical tone can be heard, not only by placing the ear immediately behind the part receiving the beam, but by placing it against any portion of the sheet, even though this may be a foot or more from the place acted upon by the light.

2. When the beam is thrown upon the diaphragm of a "Blake Transmitter," a loud musical tone is produced by a telephone connected in the same galvanic circuit with the carbon button (A), Fig. 4. Good effects are also produced when the carbon button (A) forms with the battery (B) a portion of the primary circuit of an induction coil, the telephone (C) being placed in the secondary circuit. In these cases the wooden box and mouth-piece of the transmitter should be

removed, so that no air cavities may be left on either side of the diaphragm.

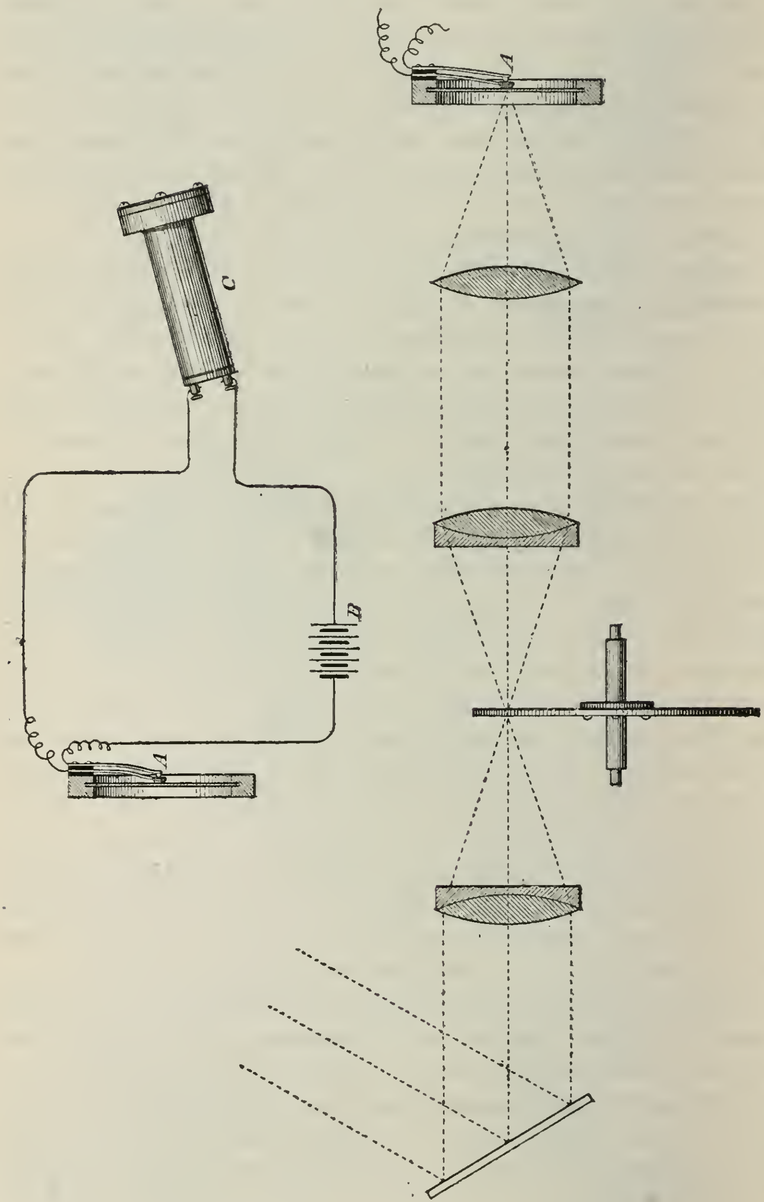

Fig. 4.

It is evident, therefore, that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, independently of any expansion and contraction of the air confined in the cavity behind the diaphragm.

Lord Rayleigh has shown mathematically that a to-and-fro vibration, of sufficient amplitude to produce an audible sound, would result from a periodical communication and abstraction of heat, and he says: "We may conclude, I think, that there is at present no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating." (*Nature*, xxiii, p. 274.) Mr. Preece, however, seeks to prove that the sonorous effects cannot be explained upon this supposition, but his experimental proof is inadequate to support his conclusion. Mr. Preece expected that if Lord Rayleigh's explanation was correct, the expansion and contraction of a thin strip under the influence of an intermittent beam could be caused to open and close a galvanic circuit so as to produce a musical tone from a telephone in the circuit. But this was an inadequate way to test the point at issue, for Lord Rayleigh has shown (*Proc. Royal Society*, 1877) that an audible sound can be produced by a vibration whose amplitude is *less than a ten-millionth of a centimetre*, and certainly such a vibration as that would not have sufficed to operate a "make-and-break contact" like that used by Mr. Preece. The negative results obtained by him cannot, therefore, be considered conclusive.

The following experiments (devised by Mr. Tainter) have given results decidedly more favorable to the theory of Lord Rayleigh than to that of Mr. Preece.

1. A strip (A) similar to that used in Mr. Preece's experiment was attached firmly to the centre of an iron diaphragm (B), as shown in Fig. 5, and was then pulled taut at right angles to the plane of the diaphragm. When the intermittent beam was focussed upon the strip (A) a clear musical tone could be heard by applying the ear to the hearing-tube (C). *This seemed to indicate a rapid expansion and contraction of the substance under trial.* But a vibration of the diaphragm (B) would also have resulted if the thin strip (A) had acquired a to-and-fro motion, due either to the direct impact of the beam or to the sudden expansion of the air in contact with the strip.

2. To test whether this had been the case, an additional strip (D)

was attached by its central point only to the strip under trial, and was then submitted to the action of the beam, as shown in Fig. 6.

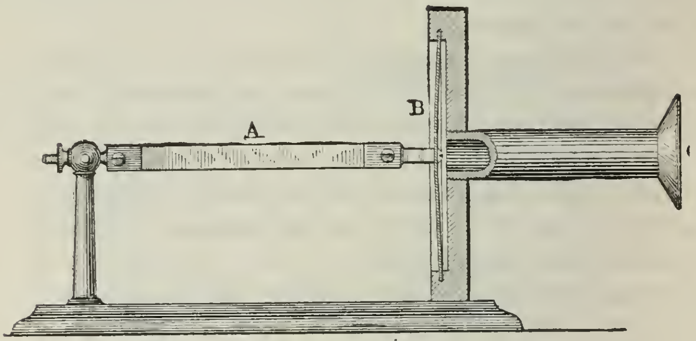

Fig. 5.

It was presumed that if the vibration of the diaphragm (B) had been due to a *pushing force* acting on the strip (A), that the addition of the strip (D) would not interfere with the effect. But if, on the other hand, it had been due to the longitudinal expansion and contraction of the strip (A), the sound would cease, or at least be reduced.

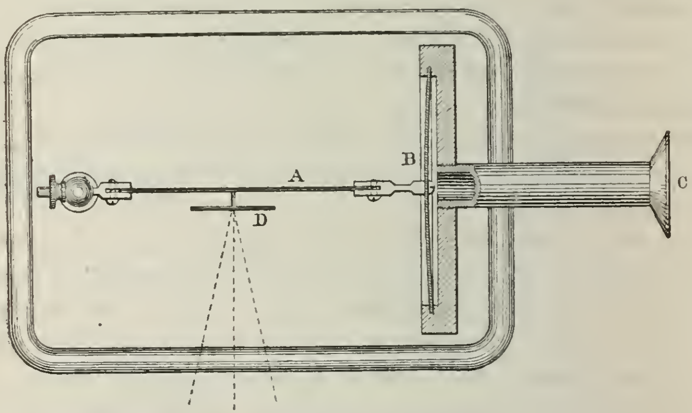

Fig. 6.

The beam of light falling upon strip (D) was now interrupted as before by the rapid rotation of a perforated disk, which was allowed to come gradually to rest. No sound was heard excepting at a certain speed of rotation, when a feeble musical tone became audible. This result is confirmatory of the first.

The audibility of the effect at a particular rate of interruption suggests the explanation that the strip D had a normal rate of vibration of its own.

When the frequency of the interruption of the light corresponded to this, the strip was probably thrown into vibration after the manner of a tuning-fork, in which case a to-and-fro vibration would be propagated down its stem or central support to the strip (A). This indirectly proves the value of the experiment.

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here, and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment.*

Experiments with Liquids.

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absorptive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light, but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment. In the experiments made in my laboratory a very long test-tube was filled with the liquid under examination, and a flexible rubber tube was slipped over the mouth far enough down to prevent the possibility of any light reaching the vapor above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focussed upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

Results.

Clear water, ...	No sound audible.
Water discolored by ink, . . .	Feeble sound.
Mercury, ...	No sound heard.
Sulphuric ether,* . .	Feeble but distinct sound.
Ammonia, . . .	" " "
Ammonio-sulphate of copper, .	" " "
Writing ink, . . .	" " "
Indigo in sulphuric acid, .	" " "
Chloride of copper,* . .	" " "

*Carbon and thin microscope glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy (*Comptes Rendus*, vol. xci, p. 595). All these substances have since yielded sounds under more careful conditions of experiment.

The liquids distinguished by an asterisk gave the best sounds.

Acoustic vibrations are always much enfeebled in passing from liquids to gases, and it is probable that a form of experiment may be devised which will yield better results by communicating the vibrations of the liquid to the ear through the medium of a solid rod.

Experiments with Gaseous Matter.

On the 29th of November, 1880, I had the pleasure of showing to Prof. Tyndall in the laboratory of the Royal Institution the experiments described in the letter to Mr. Tainter from which I have quoted above, and Prof. Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapor of sulphuric ether (a good absorbent of heat), and another with the vapor of bisulphide of carbon (a poor absorbent), and he predicted that if any sound was heard it would be louder in the former case than in the latter. The experiment was immediately made, and the result verified the prediction.

Since the publication of the memoirs of Röntgen* and Tyndall† we have repeated these experiments, and have extended the inquiry to a number of other gaseous bodies, obtaining in every case similar results to those noted in the memoirs referred to.

The vapors of the following substances were found to be highly sonorous in the intermittent beam: Water vapor, coal gas, sulphuric ether, alcohol, ammonia, amylene, ethyl bromide, diethylamene, mercury, iodine and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen.

I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition (solid, liquid and gaseous), and the probability is therefore very greatly increased that sonorousness under such circumstances will be found to be a universal property of matter.

Upon Substitutes for Selenium in Electrical Receivers.

At the time of my communication to the American Association the loudest effects obtained were produced by the use of selenium, arranged

* *Ann. der Phys. und Chem.*, 1881, No. 1, p. 155.

† *Proc. Roy. Soc.*, vol. xxxi, p. 307.

in a cell of suitable construction, and placed in a galvanic circuit with a telephone. Upon allowing an intermittent beam of sunlight to fall upon the selenium a musical tone of great intensity was produced from the telephone connected with it.

But the selenium was very inconstant in its action. It was rarely, if ever, found to be the case that two pieces of selenium (even of the same stick) yielded the same results under identical circumstances of annealing, etc. While in Europe last autumn, Dr. Chichester Bell, of University College, London, suggested to me that this inconstancy of result might be due to chemical impurities in the selenium used. Dr. Bell has since visited my laboratory in Washington, and has made a chemical examination of the various samples of selenium I had collected from different parts of the world. As I understand it to be his intention to publish the results of this analysis very soon, I shall make no further mention of his investigation than to state that he has found sulphur, iron, lead and arsenic in the so-called "selenium," with traces of organic matter; that a quantitative examination has revealed the fact that sulphur constitutes nearly one per cent. of the whole mass, and that when these impurities are eliminated the selenium appears to be more constant in its action and more sensitive to light.

Prof. W. G. Adams* has shown that tellurium, like selenium, has its electrical resistance affected by light, and we have attempted to utilize this substance in place of selenium. The arrangement of cell (shown in Fig. 7) was constructed for this purpose in the early part of 1880; but we failed at that time to obtain any indications of sensitiveness with a reflecting galvanometer. We have since found, however, that when this tellurium spiral is connected in circuit with a galvanic battery and telephone, and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone. The audible effect is much increased by placing the tellurium cell with the battery in the primary circuit of an induction coil, and placing the telephone in the secondary circuit.

The enormously high resistance of selenium and the extremely low resistance of tellurium suggested the thought that an alloy of these two substances might possess intermediate electrical properties. We have accordingly mixed together selenium and tellurium in different proportions, and, while we do not feel warranted at the present time

* *Proc. Roy. Soc.*, vol. xxiv, p. 163.

in making definite statements concerning the results, I may say that such alloys have proved to be sensitive to the action of light.

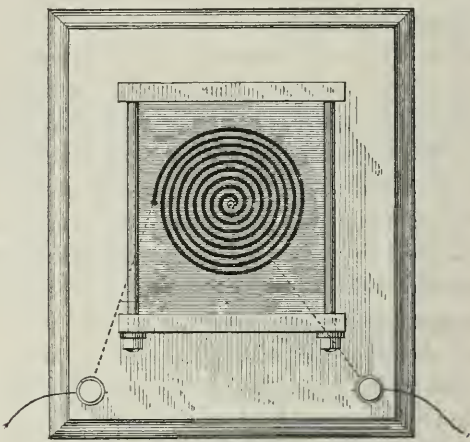

Fig. 7.

It occurred to Mr. Tainter before my return to Washington last January that the very great molecular disturbance produced in lamp-black by the action of intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lamp-black could be employed in place of selenium in an electrical receiver. This has turned out to be the case, and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

The form of lamp-black cell we have found most effective is shown in Fig. 8. Silver is deposited upon a plate of glass, and a zigzag line is then scratched through the film, as shown, dividing the silver surface into two portions insulated from one another, having the form of two combs with interlocking teeth.

Each comb is attached to a screw-cup, so that the cell can be placed in an electrical circuit when required. The surface is then smoked until a good film of lamp-black is obtained, filling the interstices between the teeth of the silver combs. When the lamp-black cell is connected with a telephone and galvanic battery, and exposed to the influence of an intermittent beam of sunlight, a loud musical tone is produced by the telephone. This result seems to be due rather to the physical condition than to the nature of the conducting material

employed, as metals in a spongy condition produce similar effects. For instance, when an electrical current is passed through spongy platinum while it is exposed to intermittent sunlight, a distinct musical tone is produced by a telephone in the same circuit. In all such cases the effect is increased by the use of an induction coil, and the sensitive cells can be employed for the reproduction of articulate speech as well as for the production of musical sounds.

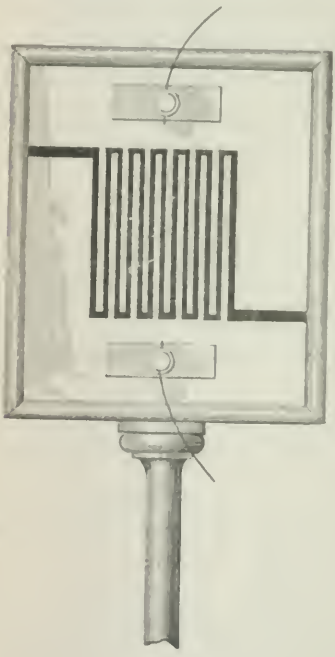

Fig. 8.

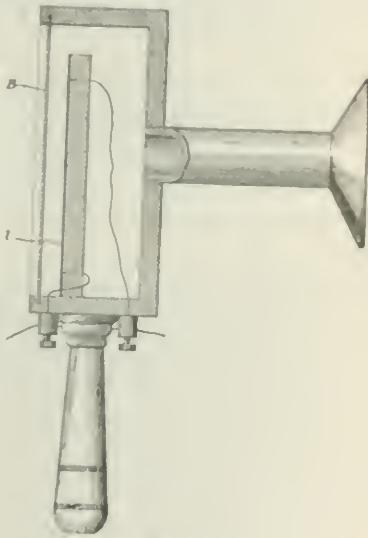

Fig. 9.

We have also found that loud sounds are produced from lamp-black by passing through it an intermittent electrical current, and that it can be used as a telephonic receiver for the reproduction of articulate speech by electrical means.

A convenient mode of arranging a lamp-black cell for experimental purposes is shown in Fig. 9. When an intermittent current is passed through the lamp-black, (*A*), or when an intermittent beam of sunlight falls upon it through the glass plate, *B*, a loud musical tone can be heard by applying the ear to the hearing-tube, *C*. When the light and the electrical current act simultaneously, two musical tones

are perceived, which produce beats when nearly of the same pitch. By proper arrangements a complete interference of sound can undoubtedly be produced.

Upon the Measurement of the Sonorous Effects Produced by Different Substances.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment, and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects, but as our researches are not yet complete, I shall confine myself to a simple description of some of the forms of apparatus we have devised.

When a beam of light is brought to a focus by means of a lens, the beam diverging from the focal point becomes weaker as the distance increases in a calculable degree. Hence, if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity, we can calculate their relative sonorous powers.

Preliminary experiments were made by Mr. Tainter during my absence in Europe to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between different substances in this respect.

Table of Distances from Focal Point of Lens at which Sounds become Inaudible with Different Substances.

Zinc diaphragm (polished),				1·51	m.
Hard rubber diaphragm,				1·90	"
Tin-foil	"			2·00	"
Telephone	"	(Japanned iron),		2·15	"
Zinc	"	(unpolished),		2·15	"
White silk	(In receiver shown in Fig. 1),			3·10	"
White worsted	"	"	"	4·01	"
Yellow worsted	"	"	"	4·06	"
Yellow silk	"	"	"	4·13	"
White cotton-wool	"	"	"	4·38	"
Green silk	"	"	"	4·52	"

Blue worsted	(In receiver shown in Fig. 1),			4·69 m.
Purple silk	"	"	"	4·82 "
Brown silk	"	"	"	5·02 "
Black silk	"	"	"	5·21 "
Red silk	"	"	"	5·24 "
Black worsted	"	"	"	6·50 "
Lamp-black. In receiver the limit of audibility could not be determined on account of want of space. Sound perfectly audible at a distance of				10·00 "

Mr. Tainter was convinced from these experiments that this field of research promised valuable results, and he at once devised an apparatus for studying the effects, which he described to me upon my return from Europe. The apparatus has since been constructed, and I take great pleasure in showing it to you to-day.

(1.) A beam of light is received by two similar lenses (A B, Fig. 10), which bring the light to a focus on either side of the interrupting disk (C). The two substances, whose sonorous powers are to be compared, are placed in the receiving vessels (D E) (so arranged as to expose equal surfaces to the action of the beam) which communicate by flexible tubes (F G) of equal length with the common hearing-tube (H). The receivers (D E) are placed upon slides, which can be moved along the graduated supports (I K). The beams of light passing through the interrupting disk (C) are alternately cut off by the swinging of a pendulum, (L). Thus a musical tone is produced alternately from the substance in D and from that in E. One of the receivers is kept at a constant point upon its scale, and the other receiver is moved towards or from the focus of its beam until the ear decides that the sounds produced from D and E are of equal intensity. The relative positions of the receivers are then noted.

(2.) Another method of investigation is based upon the production of an interference of sound, and the apparatus employed is shown in Fig. 11. The interrupter consists of a tuning-fork, (A), which is kept in continuous vibration by means of an electro-magnet (B). A powerful beam of light is brought to a focus between the prongs of the tuning-fork (A), and the passage of the beam is more or less obstructed by the vibration of the opaque screens (C D) carried by the prongs of the fork. As the tuning-fork (A) produces a sound by its own vibration, it is placed at a sufficient distance away to be inaudible through the air, and a system of lenses is employed for the purpose of bringing

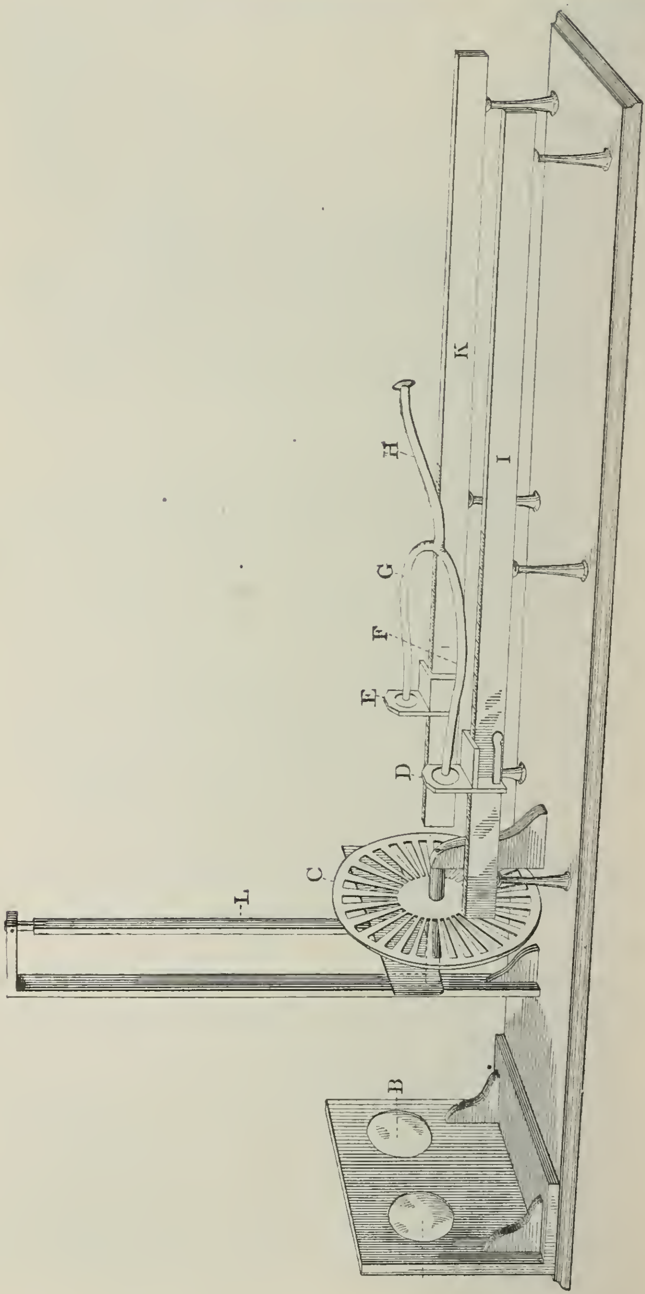

Fig. 10.

the undulating beam of light to the receiving lens (E) with as little loss as possible. The two receivers (F G) are attached to slides (H I), which move upon opposite sides of the axis of the beam, and the receivers are connected by flexible tubes of unequal length (K L), communicating with the common hearing-tube (M). The length of the tube (K) is such that the sonorous vibrations from the receivers (F G) reach the common hearing-tube, (M), in opposite phases. Under these circumstances silence is produced when the vibrations in the receivers (F G) are of equal intensity. When the intensities are unequal, a residual effect is perceived. In operating the instrument the position of the receiver (G) remains constant, and the receiver (F) is moved to or from the focus of the beam until complete silence is produced. The relative positions of the two receivers are then noted.

(3.) Another mode is as follows: The loudness of a musical tone produced by the action of light is compared with the loudness of a tone of similar pitch produced by electrical means. A rheostat introduced into the circuit enables us to measure the amount of resistance required to render the electrical sound equal in intensity to the other.

(4.) If the tuning-fork (A) in Fig. 11 is thrown into vibration by an undulatory instead of an intermittent current passed through the electro-magnet (B), it is probable that a musical tone, electrically produced in the receiver (F) by the action of the same current, would be found capable of extinguishing the effect produced in the receiver (G) by the action of the undulatory beam of light, in which case it should be possible to establish an acoustic balance between the effects produced by light and electricity by introducing sufficient resistance into the electric circuit.

Upon the Nature of the Rays that Produce Sonorous Effects in Different Substances.

In my paper read before the American Association last August and in the present paper I have used the word "light" in its usual rather than its scientific sense, and I have not hitherto attempted to discriminate the effects produced by the different constituents of ordinary light, the thermal, luminous and actinic rays. I find, however, that the adoption of the word "photophone" by Mr. Tainter and myself has led to the assumption that we believed the audible effects discovered by us to be due entirely to the action of luminous rays. The mean-

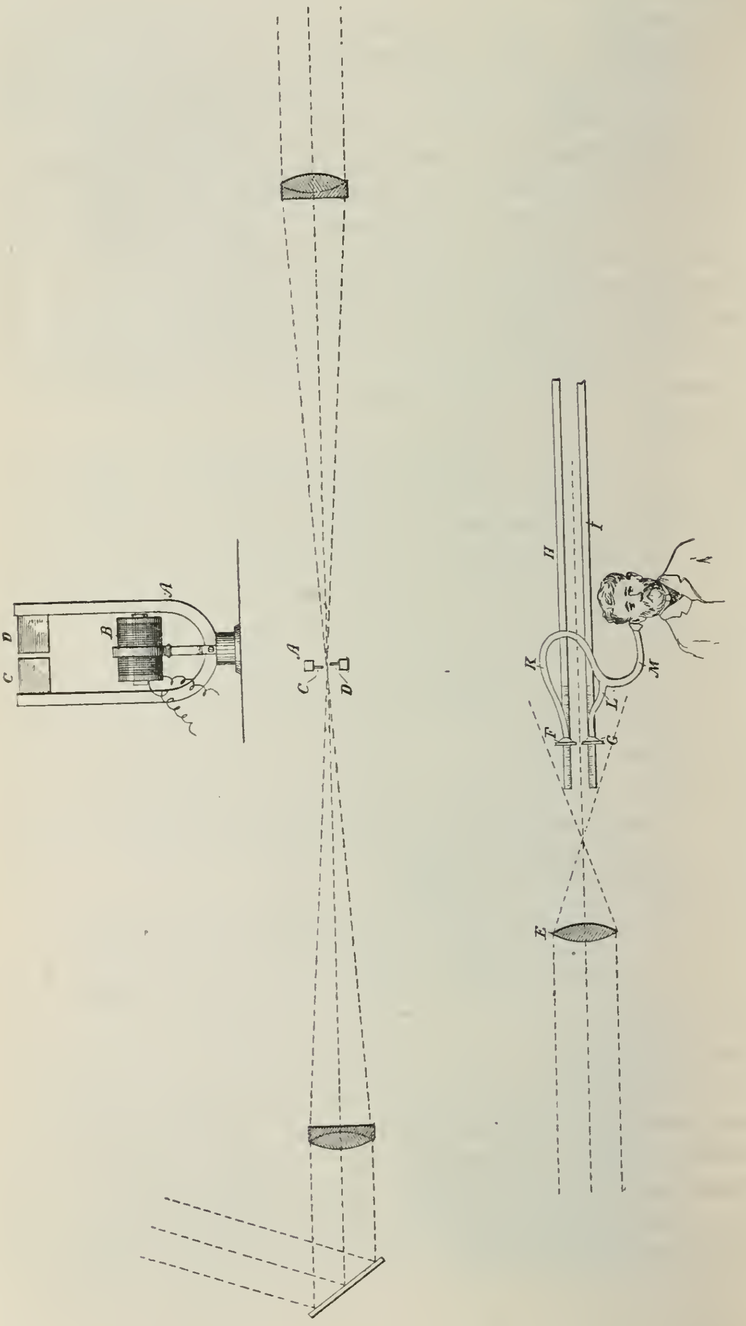

Fig. 11.

ing we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage, quoted from my Boston paper:

Fig. 12.

"Although effects are produced as above shown by forms of radiant energy, which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone' *because an ordinary beam of light contains the rays which are operative.*"

To avoid in future any misunderstandings upon this point we have decided to adopt the term "*radiophone,*" proposed by M. Mercadier, as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone, photophone* and *actinophone* to apparatus for the production of sound by thermal, luminous or actinic rays, respectively.

M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum. (*Comptes Rendus*, Dec. 6th, 1880.) We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier. A beam of sunlight was reflected from a heliostat (A, Fig. 12) through an achromatic lens (B), so as to form an image of the sun upon the slit (C). The beam then passed through another

achromatic lens (D) and through a bisulphide of carbon prism (E), forming a spectrum of great intensity, which, when focused upon a screen, was found to be sufficiently pure to show the principal absorption lines of the solar spectrum. The disk-interrupter (F) was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the receiver (G), which was so arranged that the lamp-black surface exposed was limited by a slit, as shown. Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver (G) gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to decrease, and then stopped so suddenly that a very slight motion of the receiver (G) made all the difference between almost maximum sound and complete silence.

(2.) The lamp-blacked wire gauze was then removed and the interior of the receiver (G) was filled with red worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

(3.) Upon substituting green silk for red worsted, the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

(4.) Some hard-rubber shavings were now placed in the receiver (G). The limits of audibility appeared to be on the one hand the junction of the green and blue, and on the other the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red, and to his ear the maximum was about the junction of the red and orange.

(5.) A test-tube containing the vapor of sulphuric ether was then substituted for the receiver (G). Commencing at the violet end, the test-tube was gradually moved down the spectrum and out into the ultra-red without audible effect, but when a certain point far out in the ultra-red was reached a distinct musical tone suddenly made its

appearance, which disappeared as suddenly on moving the test-tube a very little further on.

(6.) Upon exploring the spectrum with a test-tube containing the vapor of iodine, the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

(7.) A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were obtained in all parts of the visible spectrum, but no sounds were observed in the ultra-red.

The maximum effect seemed to me to be in the blue. The sounds were well marked in all parts of the violet, and I even fancied that the audible effect extended a little way into the ultra-violet, but of this I cannot be certain. Upon examining the absorption spectrum of peroxide of nitrogen it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption lines made their appearance.

(8.) The spectrum was now explored by a selenium cell, and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red. The audible effect extended a little way into the ultra-red on the one hand, and up as high as the middle of the violet on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that *the nature of the rays that produce sonorous effects in different substances depends upon the nature of the substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.*

The Spectrophone.

Our experiments upon the range of audibility of different substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis, which was described and exhibited to the Philosophical Society of Washington.* The eye-piece of a spectroscope is removed, and sensitive substances are placed in the focal point of the instrument behind an opaque diaphragm containing a slit. These substances are put in communication with the

* Proc. of Phil. Soc. of Washington, April 16, 1881.

ear by means of a hearing-tube, and thus the instrument is converted into a veritable "spectrophone," like that shown in Fig. 13.

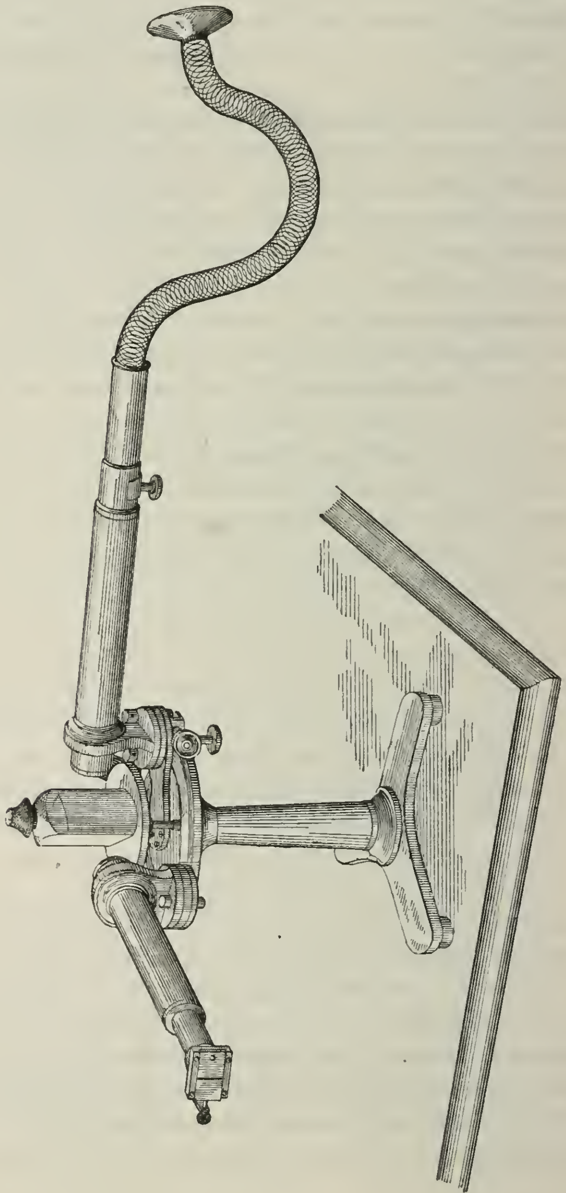

Fig. 13.

Suppose we smoke the interior of our spectrophonic receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum (visible and invisible), except the ultra-violet. Now pass a rapidly-interrupted beam of light through some substance whose absorption spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption bands. Of course, the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum: but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum, lamp-black alone may be used in the spectrophonic receiver. Indeed, the sounds produced by this substance in the ultra-red are so well marked as to constitute our instrument a most reliable and convenient substitute for the thermo-pile. A few experiments that have been made may be interesting.

(1.) The interrupted beam was filtered through a saturated solution of alum. Result: The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

(2.) A thin sheet of hard rubber was interposed in the path of the beam. Result: Well-marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time. (See diagram of results in Fig. 14.)

(3.) A solution of ammonia-sulphate of copper was tried. Result: When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear, however, the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination, but I do not wish you to understand

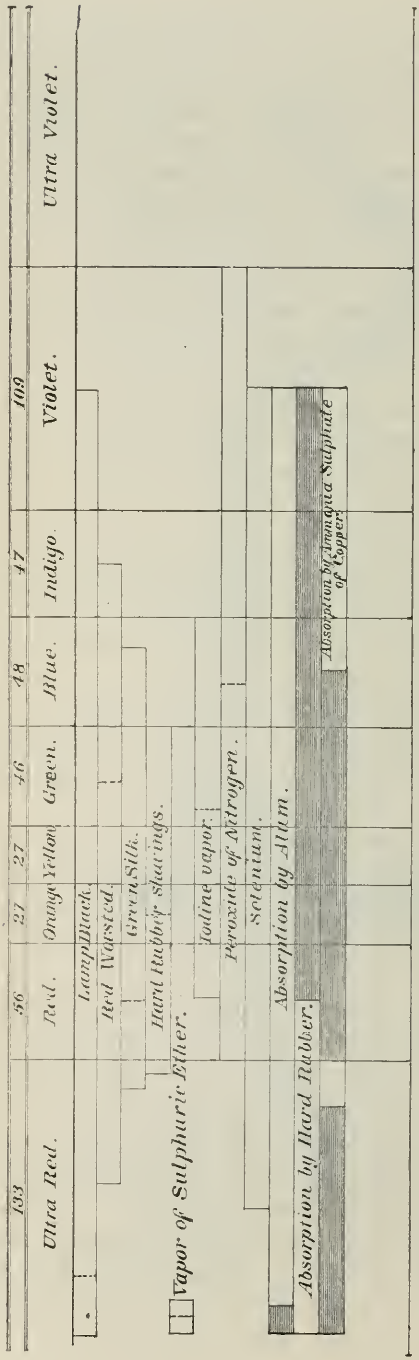

Fig. 14.

that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man, and I feel that *our* first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognize the fact that the spectrophone must ever remain a mere adjunct to the spectroscope, but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption spectra in the ultra-red.

Tunnel of the English Channel. — The preliminary works of the tunnel which is to unite England and France have presented very satisfactory results. The engineers have sunk a shaft to the stratum in which they propose to bore the tunnel, and they are now engaged upon a new shaft for receiving all the boring apparatus. They expect to bore at least $2\frac{1}{2}$ miles within 18 months and to finish the work in 10 years.—*L'Ingen. Univ.* C.

AN ACCOUNT OF THE EXPERIMENTS MADE IN MULHOUSE, GERMANY, BY A COMMITTEE OF THE INDUSTRIAL SOCIETY OF THAT CITY, ON A CORLISS STEAM ENGINE, TO DETERMINE ITS ECONOMIC PERFORMANCE WITH AND WITHOUT STEAM-JACKETING.

By Chief Engineer ISHERWOOD, U. S. Navy.

(Continued from page 371.)

RESULTS OF THE EXPERIMENTS.

In all the experiments except *H*, *I*, *J* and *K*, the cut-off was fixed, so that the measure of expansion with which the steam was used did not vary during each experiment. In experiments *H*, *I*, *J* and *K*, the cut-off was variable by the action of the governor, but the uniformity of the load and the steadiness of the steam pressure restricted the point of cutting off within very narrow limits.

The economy of the performance in the different experiments may be compared for the total horse-power and for the net horse-power developed by the engine, the units of heat consumed per hour per horse-power being taken as the measure of the economy. The total horse-power represents the entire dynamic effect of the steam, including overcoming the external load and the internal resistance of the back pressure against the piston and all the friction resistances. The net horse-power represents that portion of the entire dynamic effect of the steam which is expended in overcoming the external load and the friction of that load; it is exclusive of the dynamic effect expended in overcoming the internal resistance of the back pressure against the piston, and of the friction of the unloaded engine. The net horse-power is the only portion of the dynamic effect of the steam which is commercially valuable, and its greater or less cost is the only economic problem in this relation to the user of steam power.

Of the economy due to the different measures of expansion with which the steam was used. In the case of experiments *A*, *B* and *C*, where there was no steam present in the cylinder jackets or piston, the steam was expanded 12·3978 times in experiment *A*, and 7·9033 times in experiments *B* and *C*. Now as experiment *C* was a repetition of

experiment B, the mean of the economic results of the two may be taken as the cost of the power in units of heat consumed per hour. That mean for the total horse-power is $\left(\frac{26143{\cdot}4461+25621{\cdot}4174}{2}=\right)$ 25882·4317 units. The cost of the total horse-power in experiment A was 26731·8267 units of heat consumed per hour; hence, for the total horse-power under the conditions of these experiments, expand- the steam 12·3978 times, gave $\left(\frac{26731{\cdot}8267-25882{\cdot}4317\times100}{25882{\cdot}4317}=\right)$ 3·2817 per centum less economy than expanding it 7·9033 times.

Making the same comparison for the net horse-power, we have for its cost, as the mean of the experiments B and C,

$$\left(\frac{32440{\cdot}2587+31735{\cdot}4286}{2}=\right)$$

32087·8436 units of heat consumed per hour, the steam being expanded 7·9033 times. In experiment A, in which the steam was expanded 12·3978 times, the cost of the net horse-power was 37285-·6168 units of heat consumed per hour; hence, for the net horse-power under the conditions of these experiments, expanding the steam 12·3978 times gave $\left(\frac{37285{\cdot}6168-32087{\cdot}8436\times100}{32087{\cdot}8436}=\right)$16·1986 per centum less economy than expanding it 7·9033 times.

In the case of experiments E and F, where steam was present in the cylinder jackets, but not in the piston, the steam was expanded 10·8259 times in experiment E, and 5·7037 times in experiment F. The cost of the total horse-power in experiment E was 19452·7617 units of heat consumed per hour, and in experiment F 20230·7613 units; hence, for the total horse-power under the conditions of these experiments, expanding the steam 10·8259 times gave

$$\left(\frac{20230{\cdot}7613-19452{\cdot}7617\times100}{20230{\cdot}7613}=\right)$$

3.8456 per centum more economy than expanding it 5·7037 times.

Making the same comparison for the net horse-power, we have for its cost in experiment E 25542·3868 units of heat consumed per hour, and in experiment F 24223·0957 units; hence, for the net horse-power under the conditions of these experiments, expanding the steam 10·8259 times gave $\left(\frac{25542{\cdot}3868-24223{\cdot}0957\times100}{24223{\cdot}0957}=\right)$5·4464 per centum less economy than expanding it 5·7037 times.

In the case of experiments *G*, *H*, *I*, *J*, *K* and *L*, where steam was present in the cylinder jackets and in the piston, the steam was expanded a different number of times in each experiment, varying from 5·7037 to 10·8259 times. The following table shows the relative costs of the total horse-power and of the net horse-power for these different measures of expansion in units of heat consumed per hour, assuming for unity the cost of the horse-power with the steam expanded 5·7037 times.

Designation of experiment	Number of times the steam was expanded	Relative costs of the horse-power in units of heat consumed per hour	
		Total horse-power	Net horse-power
L	5·7037	1·0000	1·0000
K	5·8670	0·9639	0·9649
J	6·4182	0·9712	0·9758
I	6·8471	0·9692	0·9797
H	7·9033	0·9384	0·9719
G	10·8259	0·9199	0·9984

The general results as regards the different measures of expansion with which the steam was used are: 1st. That with saturated steam, steam jacketing allows a greater gain to be realized with large measures of expansion than can be had without steam jacketing. In other words, expansion can beneficially be carried farther with steam jacketing than without. 2d. That with steam jacketing under the conditions of these experiments the total horse-power is obtained with slightly increasing economy as the measure of expansion increases; but that, on the contrary, a small economic loss is experienced as regards the net horse-power with each increase of the measure of expansion beyond about 5¾ times.

Without steam jacketing and using saturated steam, an economic loss was sustained for both the total and the net horse-power, small for the first and large for the last, when the measure of expansion was increased from 7·9033 to 12·3978 times.

In connection with steam jacketing and different measures of expansion, it will be observed that the per centum of the steam evap-

orated in the boiler, condensed in the steam jackets, increased regularly as the measure of expansion increased, rising from 4·6050 per centum when the steam was expanded 5·7037 times (experiment *L*) to 6·4641 when the steam was expanded 10·8259 times (experiment *G*). This was as it should be, the greater cylinder refrigeration accompanying the greater measure of expansion, must necessarily draw from the steam jackets a greater proportion of heat. The fact that the jackets furnish a reservoir of heat for counteracting the greater cylinder refrigeration due to the greater measures of expansion, allows these greater measures to be more economically beneficial with steam jacketing than without it, as shown by the experiment.

The results of these experiments show that, as regards economy of fuel, there is no gain in a variable cut-off actuated by the governor of the engine between the limits of expansion employed. The variable cut-off in these cases was only useful for the graduation of the power which it probably accomplished somewhat more promptly than could have been effected by connecting the governor with a throttle valve.

It is necessary to here caution the reader that the condition of constant piston speed in the same cylinder, under which these experiments were made, is exceptionally favorable for obtaining relatively the highest economic results for the greater measures of expansion. This uniformity of piston speed was maintained by the action of the spare duplicate engine connected on the same shaft and supplying the complement of power for equal speed, let the power of the experimental engine vary as it might owing to the variations in the measures of expansion with which the steam of the same boiler pressure was used.

In regular practice, with the same cylinder, a constant boiler pressure, and a constant load, the piston speed decreases as the measure of expansion increases, because the piston pressure becomes correspondingly less and less. The weight of steam condensed in the cylinder per hour in addition to the condensation due to the development of the total power, and caused by the variations in the temperature of the interior metallic surfaces of the cylinder during a stroke of the piston, being nearly constant in these cases, while the weight of steam evaporated per hour in the boiler is smaller and smaller as the power becomes smaller and smaller, the steam condensed in the cylinder becomes necessarily a larger proportion of the steam evaporated in the boiler, and the economy of the performance correspondingly decreases

with every increase in the measure of expansion. Had the experiments been made under these unavoidable conditions of regular practice, the economic results of the greater measures of expansion would have been much less than they were. There would have probably been some loss in economic effect for the total horse-power, and certainly a marked loss for the net horse-power in all the cases where the measure of expansion exceeded the lowest employed, namely: 5·7037 times. If we seek to preserve the economic effect of the higher measures of expansion when a constant piston speed is maintained, by preserving that speed in a cylinder of corresponding size to give the same development of power with constant load and the same boiler pressure, there still results the larger condensation of steam per hour in the larger cylinder due to the greater extent of its interior surfaces.

Of the economy due to steam of the boiler pressure in the cylinder jackets and piston. To ascertain the economic gain due to steam jacketing the cylinder and piston with steam of the boiler pressure, the cost of the total horse-power must be compared in the case of the jacketing with the similar cost in the case of no jacketing, taking care that in both cases the initial steam pressure on the piston and the mean back pressure against it, the measure of expansion with which the steam was used, the speed of the piston, and the total horse-power developed by the engine, are about the same.

For the determination without steam in the cylinder jackets and piston, we have the mean of experiments *B* and *C*, giving for the initial pressure on the piston 71·0145 pounds per square inch above zero, for the back pressure against the piston 3·1170 pounds per square inch above zero, for the measure of expansion with which the steam was used 7·9033 times, for the speed of the piston 49·2003 double strokes per minute, and for the total horse-power developed 144·0238.

For the determination with steam in the cylinder jackets and piston, we have the results of the comparable experiment *H* in which the initial pressure on the piston was 71·2090 pounds per square inch above zero, the back pressure against the piston 3·1599 pounds per square inch above zero, the measure of expansion 7·9033 times, the speed of the piston 51·0981 double strokes per minute, and the total horse-power 151·5674.

The mean of experiments *B* and *C* gave for the cost of the total horse-power 25882·4317 units of heat consumed per hour. Experi-

ment *H* gave for the cost of the total horse-power 19381·3170 units of heat consumed per hour; consequently steam jacketing the cylinder and piston produced an economic gain of

$$\left(\frac{25882{\cdot}4317 - 19381{\cdot}3170 \times 100}{25882{\cdot}4317} = \right)$$

25·1179 per centum.

It is to be regretted that the experiments without steam jacketing were not more numerous and made with more varied measures of expansion, so that a greater number of comparisons might have been obtained with the experiments in which steam jacketing was employed. The want of more comparable experiments may be supplied, but not with strict accuracy, by comparing the mean of the results given by experiments *A*, *B* and *C*, in which there was no steam in the cylinder jackets and piston, with the mean of the results given by experiments *G* and *H*, in which steam of boiler pressure was in the cylinder jackets and piston, the average measure of expansion for experiments *A*, *B* and *C* being almost exactly the average for experiments *G* and *H*.

The mean results of experiments *A*, *B* and *C* are: initial pressure on the piston 71·3507 pounds per square inch above zero, back pressure against the piston 3·0733 pounds per square inch above zero, measure of expansion 9·4015 times, speed of piston 49·4423 double strokes per minute, total horse-power developed by the engine 128·4708, and units of heat consumed per hour per total horse-power 26165·5634.

The mean results of experiments *G* and *H* are: initial pressure on the piston 71·8300 pounds per square inch above zero, back pressure against the piston 3·0588 pounds per square inch above zero, measure of expansion 9·3646 times, speed of piston 50·7483 double strokes per minute, total horse-power developed by the engine 135·4667, and units of heat consumed per hour per total horse-power 19190·6949.

From the above data, steam jacketing the cylinder and piston produced an economic gain of $\left(\frac{26165{\cdot}5634 - 19190{\cdot}6949 \times 100}{26165{\cdot}5634} = \right)$ 26·6566 per centum.

Inasmuch as during the previous experiments, those made with steam of boiler pressure in the cylinder jackets and piston had the advantage of a slightly greater piston speed over those made without steam in the cylinder jackets and piston, the economic gain due to the former over the latter may be taken at 25 per centum.

In experiments G and H there were drained from the cylinder jackets alone $\left(\frac{5·1224 + 4·7990}{2} =\right)$ 4·9607 per centum of the water vaporized in the boiler: and from the piston alone $\left(\frac{1·3417 + 1·2254}{2} =\right)$ 1·2835 per centum of the water vaporized in the boiler, making a total of (4·9607 + 1·2835 =) 6·2442 per centum, of which roundly one-fifth was contributed by the piston and four-fifths by the cylinder jackets, being almost exactly in the proportion of the steam jacketed surfaces of the piston to the steam jacketed surfaces of the cylinder, showing that per unit of surface the jacketing of the piston gave about the same condensation as the jacketing of the cylinder.

If, from the weight of feed water pumped into the boiler in experiment H there be deducted the weight of water of condensation drained from the cylinder jackets and piston, there will remain 2448·4284 pounds of water which entered the cylinder per hour in the form of steam containing 2760609·1421 units of heat, so that the total horse-power in that experiment cost 18213·7395 units of heat exclusive of the units in the steam supplying the cylinder jackets and piston.

Comparing this result with the mean cost of the total horse-power in units of heat consumed per hour during experiments B and C (25882·4317 units), in which there was no steam in the cylinder jackets and piston, there is found an economic gain of

$$\left(\frac{25882·4317 - 18213·7395 \times 100}{25882·4317} =\right)$$

29·6290 per centum for the steam jacketing, showing that the condensation in the steam cylinder during experiments B and C exceeded that in experiment H by at least 29·6290 per centum of the feed water pumped into the boiler. The economy effected by the steam jacketing resulted entirely from this great lessening of the enormous cylinder condensation which always takes place in small cylinders using saturated steam with large measures of expansion.

In experiment H a condensation in the steam jackets of 6·0244 per centum of the steam evaporated in the boiler prevented a condensation in the cylinder of 29·6290 per centum of the water evaporated in the boiler. This condensation in the jackets included not only the heat imparted to the interior surfaces of the cylinder, but also the heat lost by radiation from the exterior surfaces of the cylinder jackets. As

these latter, however, were thoroughly protected by a covering of non-conducting materials, the loss from that cause must have been quite insignificant; but, whatever it was, a part of it should be deducted from the 6·0244 per centum, because, had there been no steam jackets, there would have been some, but less, radiation from the exterior surfaces of the cylinder; less, because the exterior surfaces of the cylinder and the steam pressure within it are less than for the jackets.

Again, under the conditions of ordinary practice, the steam jacketing would have given a slightly greater economy than in these experiments, because, in that case, the water of condensation from the jackets is delivered directly into the boiler with nearly the temperature of the steam in the latter having lost therein only its latent heat; while, in the case of the experiments, this water of condensation was cooled down to about the feed water temperature by being drained into measuring tanks before returning to the boiler. In experiment *H*, every pound of the water of condensation drained from the jackets thus lost about 231 units of heat; had this been saved, as it might be in regular practice, the economic gain by the steam jacketing would have been increased 1·23 per centum, so that the true gain in fuel in regular practice due to the presence of steam in the cylinder jackets and piston would have been about 26·5 per centum.

It must not be supposed that this gain is absolute and the same for all steam engines; on the contrary, it is relative to the type of engine, to the proportions of the cylinder, to the dimensions of the cylinder, to the initial pressure on the piston and the back pressure against it, to the measure of expansion with which the steam is used, to the degree of superheating the steam may possess on entering the cylinder, to the proportion of water entrained by the steam, and to the speed of piston. In brief, the gain due to steam jacketing is affected by all the causes which affect the condensation of steam in the cylinder other than the condensation due to the transmutation of the heat, thus set free, into the total horse-power developed by the expanding steam alone.

Different types of engine require, for the same cylinder, different space in the clearance and in the steam passage; they also allow a different proportion of the exterior of the cylinder to be utilized for jackets. Thus, with the same dimensions of cylinder, the area of steam jacketing may be less, and the area of the internal surface of the cylinder, including surfaces of clearance and steam passage, more with

one type than with another, in which case steam jacketing would be less economical than in the reverse case. Other things equal, the proportions of cylinder which diminish the gain due to steam jacketing are those which enclose a given space with the least superficies; for then a given mass of steam is exposed to the least condensing surface. The larger the dimensions of the cylinder, other things equal, the less the gain by steam jacketing, because the mass of steam increases as the cube of the dimensions, while its enclosing or condensing surface increases as the square only. The less the difference, other things equal, between the initial pressure on the piston and the back pressure against it, the less will be the gain by steam jacketing, as this difference is one of the causes of cylinder condensation. The greater the degree of superheating possessed by the steam, other things equal, the less will be the gain due to steam jacketing, because there will be less cylinder condensation for the jacket to act on. With a degree of superheating sufficient to prevent condensation, steam jacketing would be nugatory. The greater the proportion of water entrained by the steam, the greater will be the economic gain by steam jacketing, for the presence of water in the cylinder greatly increases the cylinder condensation, as it has to be boiled off during the expansion and the exhaust strokes largely by heat taken from the metal of the cylinder, and this deficit must be restored by the entering steam which to that extent undergoes condensation. The greater the speed of the piston, other things equal, the less will be the gain by steam jacketing; for, although the weight of steam condensed per hour, in the same cylinder, under this condition, may be nearly the same with all speeds of piston within practical limits, yet as the mass of steam passing through the cylinder in a given time will be in direct proportion to the piston speed, the cylinder condensation will be correspondingly reduced in proportion to the boiler evaporation; for example, if, with a given speed of piston, the condensation was 20 per centum of the steam evaporated in the boiler, then, with the piston speed doubled, this condensation would fall to 10 per centum, provided always that the metal of the cylinder transmitted the jacket temperature to the interior surfaces of the cylinder as rapidly as the steam came upon them. If this transmission was slower, then the reduction in the per centum of the condensation would not be so great.

Of the economy due to steam of the boiler pressure in the cylinder jackets alone — not in the piston. We have already ascertained the

economic gain due to the presence of steam of boiler pressure in the cylinder jackets and in the piston; there remains to determine this gain for the piston alone. The committee should have made a direct experiment for the purpose of ascertaining the economic efficiency of the steam jacketed surfaces of the piston, by leaving the steam out of the cylinder jackets and experimenting with it in the piston alone. Instead of this, they deducted the economic results when using steam in the cylinder jackets alone from those obtained when using it in the cylinder jackets and piston combined. This, indeed, showed how much, in the particular experimental case, the extension of the steam jacketing to the piston affected the economic gain, but it could not show the efficiency of the jacketed surfaces of the piston, *per se*. As an illustration of the fact at issue, suppose that the cylinder jackets alone were able to prevent or nearly prevent any condensation in the cylinder, then it is obvious that the addition of the jacketed surfaces of the piston could have produced no additional effect; but, if the cylinder jackets still left a considerable cylinder condensation, then the additional jacketed surfaces of the piston would have proved very efficient.

The strictly comparable experiments for determining the efficiency of the steam jacketed piston in combination with the steam jackets of the cylinder are E with G, and F with L. In E and F, only the cylinder jackets were in use; in G and L they were in use in combination with steam in the piston.

In experiment E the total horse-power cost 19452·7617 units of heat per hour; in experiment G it cost 19000·0729 units; consequently, the addition of the jacketed surfaces of the piston to those of the cylinder increased the economic gain

$$\left(\frac{19452{\cdot}7617-19000{\cdot}0729\times 100}{19452{\cdot}7617}=\right)$$

2·3271 per centum.

In experiment F the total horse-power cost 20230·7613 units of heat per hour; in experiment L it cost 20653·5712 units; consequently, the addition of the jacketed surfaces of the piston to those of the cylinder *decreased* the economic gain

$$\left(\frac{20653{\cdot}5712-20230{\cdot}7613\times 100}{20230{\cdot}7613}=\right)$$

2·0899 per centum.

The differences from the two sets of experiments being in opposite

directions, about equal, and small, show them to result from errors of observation, and, as they neutralize each other, the conclusion is warranted that the addition of the steam jacketed surfaces of the piston to those of the cylinder produced no sensible effect. As a corollary to this there follows that the cylinder jackets alone were efficient enough to prevent all or nearly all the cylinder condensation, leaving the piston jackets nugatory.

We are here met with the fact that the weight of water of condensation drained from the piston and from the cylinder jackets in experiment *G* have the relation of 1·0000 to 3·8179, and in experiment *L* the relation of 1·0000 to 3·6539, showing a greater abstraction of heat per unit of surface from the piston than from the cylinder jackets, and this heat must have been transferred to something. Now, as the experiments show it was not used to prevent cylinder condensation, it must have been conducted away by the piston-rod which was in metallic contact with the piston, extending from the latter in both directions through both ends of the cylinder, and thence into the free air, one end of the piston-rod being in metallic contact with a crosshead. This rod undoubtedly transferred the heat from the piston jackets to the external air, and thus caused the observed condensation of steam within them.

Railway Alarm-Whistle.—In order to prevent a train passing a danger signal during a fog or snow-storm without being seen by the engineer, the Southern Railway Company of France have attached to the locomotive a steam whistle, which is controlled by the signal. The whistle is connected with an insulated metallic brush placed under the engine. Between the rails there is a projecting contact bar, faced with copper, which is swept by the brush when the train passes. This contact piece is connected with the positive pole of a voltaic battery, the negative pole of which is in communication with a commutator on the signal post, from which a wire leads to the ground. When the signal is "line clear" the passage of the brush over the fixed contact produces no result; but when the signal marks "danger," the commutator brings the negative pole of the battery in direct communication with the ground, and when the brush passes over the contact the completion of the electric current causes the whistle to be sounded, so as to alarm the driver.—*L'Ingen. Univ.* C.

THE FLIGHT OF BIRDS AND THE MECHANICAL PRINCIPLES INVOLVED.

By A. C. Campbell.

The flight of birds has always been a favorite subject of investigation, and the conclusion has ever been that it is a feat of great strength.

The apparent ease and the swiftness with which the bird moves through the air cannot have failed to excite a spirit of wondering in the mind of the acute observer. How does he contrive to sustain himself against gravity, and how is it that he can acquire such great speed by means of the appliances at his command?

Are there not some hidden mechanical principles involved in his flight that lighten his task?

Before endeavoring to give the philosophy of flight, we will consider some of the mechanical effects and properties of the atmospheric air as a medium through which the bird wings his way.

The air, as we know, has weight and so it has inertia. It has perfect elasticity. Air in motion imparts its inertia to any object impeding its movement, and so arises what is understood as the force of winds.

It is a known law of winds that their pressure or force varies with the square of their velocity. Winds having velocities of 7, 14, 21, 41, 61, 82 and 92 miles per hour exert pressures respectively per square foot of 0·2, 0·9, 1·9, 7·5, 16·7, 30·7 and 37·9 pounds. From which we may conclude that a plane surface of five square feet, facing a wind of seven miles per hour, would receive a pressure of one pound. And the result would remain the same if the plane were moved at the rate of seven miles per hour against the air at a standstill. Five hundred square feet, in like manner, would support a weight of one hundred pounds and descend through the air at a rate of seven miles per hour.

Now if the plane be inclined to the direction of the wind or force of air, there would seemingly be a two-fold diminution of pressure, namely in the first place by a less proportion of air being intercepted, and secondly on account of a less proportion of pressure being

imparted. But in practice it does not hold true, since all of the particles of air are not turned sharply in a direction parallel with the plane.

After making many attempts to discover the relative pressures upon planes of variable degrees of inclination to the direction of the force of air, I finally hit upon the following simple device which gave very good results. I constructed a balance in such a way that the wind would take effect upon two plane surfaces of variable areas, and situated at variable distances from the fulcrum.

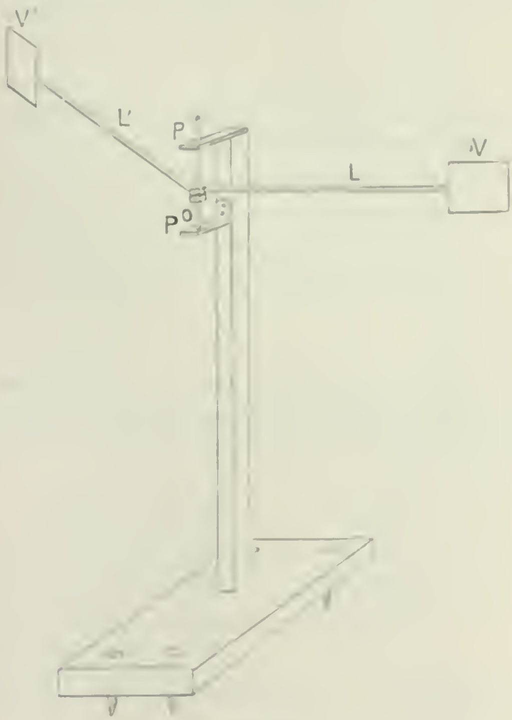

Fig. 1.

In the adjoining figure (1), V V' are the two planes fixed to the lever arms L L', the latter being firmly fixed to the spindle O by binding screws. The spindle revolves in a socket made in the lower lip or plate P, and passes through a small hole in the upper plate P'. The spindle has perfect freedom of movement and is nicely balanced.

If both planes are the same area (say ten square inches) and both

arms the same length, then there are two positions of equilibrium (supposing, all the time, that the plane V stands at right angles to the direction of the wind), namely, when both planes are at the same angle and when V' stands at an angle 30° with the direction of the wind. In other words, if (A) and (A') represent the angles which V and V' make, respectively, with the wind, then there is equilibrium when $(A) = 90°$ and $(A') = 90°$, also when $(A) = 90°$ and $(A') = 30°$, and this appears to hold true for variable pressures of wind. If (A') is made greater than 30° the pressure upon V' preponderates until the angle of about 42° is reached, when the pressure is at its maximum. As (A') is made less than 30° the pressure upon V preponderates.

The relative pressures upon the two planes, when (A') is variable, may be discovered in two ways, namely, by adjusting the lengths of the lever arms until equilibrium is attained, when the pressures would be expressed by the proportion $P : P' :: L' : L$. Likewise if the arms are retained constant and equal, and the area of the planes made variable, then the following proportion would answer $P : P' :: S' : S$, P and P' representing pressures per square foot, and S and S' the areas of the planes.

The following are a few results from actual experiment.

I fixed a plane of ten square inches at V and the same at V', then adjusted the arms at various angles, and moved the plane V' along the arm L' until the instrument balanced with the plane V standing normally to the wind.

$(A) = 90°$	$L = 19$ inches.
$A' = 30°$	$L' = 19$ "
$A' = 40°$	$L' = 15$ "
$A' = 42°$	$L' = 14·5$ "
$A' = 45°$	$L' = 16$ "

Retaining the arms at a given length and equal, the following results were obtained

$V = 10$ sq. inches.	$A = 90°$
$V' = 10$ " "	$A' = 30°$
$V' = 9$ " "	$A' = 20°$
$V' = 6$ " "	$A' = 15°$

If a plane surface of one square foot be moved normally against the air at the rate of 21 miles per hour, the pressure or resistance would be 1·9 lbs. If the plane were inclined at an angle of 30°

with the direction of its movement, the pressure would remain the same, but the buoyancy against gravity would be expressed by 1·9 cos. 30° = 1·64 lbs. while the horizontal resistance would be 1·9 sin. 30° = 0·95.

If the plane be inclined to within 15° of the direction of movement, it will receive a normal pressure of $\frac{6}{10}$ of 1·9 lbs. = 1·14 lbs., 1·14 sin. 15° = 0·295 lb., 1·14 cos. 15° = 1·1 lb. As the plane becomes nearly parallel with the direction of the force of air, a less proportion of it is intercepted and imparted, but as much as is received by the plane, the greater part gives an upward buoyancy, while a very small horizontal resistance is offered. If it be inclined to 5° then the proportion would be horizon. resis. : vertical buoyancy :: 1 : 11·43.

A bird in his flight moves against the air with such velocity that he needs to make but little effort to sustain himself against gravity, so allowing the greater portion of his strength for propulsion.

A plane moving horizontally through the air, and with such velocity and inclination that the component of the lifting force is equal to the force of gravity, then it will continue to move in the same horizontal direction. And we may say, generally, that a plane moving in any direction will continue to move in the same direction, provided, always, that the component of the force of air at right angles to the path, and the component of gravity also at right angles to the path, are in equilibrium. The component of gravity parallel with the path will accelerate or retard as the plane is inclined downward or upward.

Velocity is power or advantage by whatsoever means attained, whether by force of gravity, by force of air, or (in the case of the bird) by muscular effort. A bird moving against a wind derives a two-fold advantage, namely, from the combined velocities of the wind and his bodily movement.

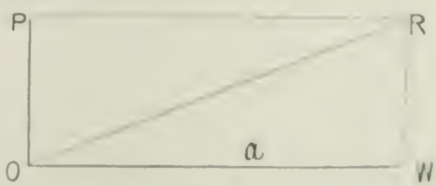

Fig. 2.

Some strange effects of the force of winds arise from combined movements.

Let $W\ O$ (Fig. 2) represent the velocity and direction of a wind, and $O\ P$ the velocity and direction of a plane maintaining a position at right angles to its path. Let $\alpha = 30°$. It is evident that such combined movements are equivalent to a movement in velocity and direction marked by $O\ R$, supposing the air at a stand-still. But it has been demonstrated by experiment that a plane, holding an angle of 30° with its path, receives the same normal pressure as when at an angle of 90°.

$O\ R = 2\ P\ O$, and the pressures being in the ratio of the squares of the velocities, would be as 1:4.

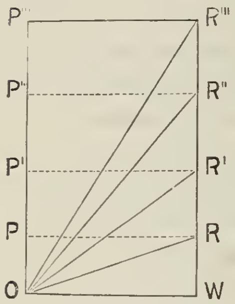

Fig. 3.

The angle (α) and the directions $O\ P$ and $W\ O$ may vary, yet the demonstration will hold true, provided the correct value is known of the normal pressure upon the plane at the angle (α) to its resultant path.

The vexed question of the ice yacht comes under the head of combined movements. Let $W\ O$ (Fig. 3) be the velocity and direction of a wind taking effect upon a plane occupying the position $O\ R$, and (α) the angle between $W\ O$ and $O\ R$. Let $O\ P$ be the path of the plane.

It matters not whether the wind move against the plane, or the plane move with the same velocity against the air at a stand-still. It is evident that if a plane move parallel with itself it will encounter no resistance of air. Passing from O to R is the two-fold movement $O\ W$ and $O\ P$. But $W\ O$ is the velocity and direction of a wind which, when substituted for the movement of the plane, makes it equivalent to the plane moving from O to P in the same time that the air passes from W to O. Likewise with $O\ R'$, $O\ R''$, $O\ R'''$, $O\ R''''$, etc. Every possible case may be referred to the parallelogram and demonstrated in like manner, remembering that the pressure is at all times normal to the surface of the plane.

A plane at an angle of 30° with the direction of a wind receives the same pressure as when at 90°, but if R represent the velocity of wind, R sin. α would express the velocity with which either plane would move in a direction perpendicular to the plane, supposing no resistance. $R \sin. 90° = R$. $R \sin. 30° = \frac{1}{2} R$.

If the inclined plane move with the wind, of course the velocity would be the same as that of the wind, but the pressure tending to

urge the plane in that direction would be one-half of that taking effect upon the normal plane, which is as it should be, since only one-half as much air is intercepted by the inclined plane.

A force of air coming in contact with an inclined plane is turned with the plane, and so long as the force continues there is a stratum of air passing down and parallel with the surface, and the constantly approaching air, instead of coming in contact with, and imparting its force directly to the plane, encounters and imparts its force to a belt of air; so the initial force of air is resolved into a force of pressure of an elastic medium acting at all times normally to the surface.

Each and every particle of air having a certain quantity of accumulated energy will impart its full effect of pressure, either directly or indirectly, gradually or abruptly, provided such particles are turned on account of the plane, more or less from their initial direction.

They may impart their effect directly by coming immediately in contact with the plane, or indirectly by imparting their pressure to other particles, and they in turn to others or a series of particles.

They may impart their effect gradually by a gradual change of direction, or abruptly by an abrupt change of direction. The quantity of pressure imparted by every particle is dependent upon the degree of change of direction.

It is a principle of mechanics that any force or forces will seek paths of most ready relief, and the principle should hold true in the case of a force of air intercepted by a surface.

A particle may begin to change from its initial direction at some distance before reaching the plane, and altogether there would be a system of arrangement and movement of the particles, arising from the pent up forces seeking paths of least resistance. After the system has become thoroughly established there is a constant minimum pressure imparted to the intervening object.

If a surface be brought instantly into cross section with a force of air, there will be an impulsive force imparted from the suddenly arrested movement of the column of air intercepted. From this there will arise a compression of air until its inertia permits it to escape from the surface and establish the system of movement referred to. The pressure is greatly augmented but of short duration.

We will suppose a flow of air to be passing through a long tube.

Now, if the discharging end be suddenly closed, the air coming in contact with the closed end will impart its inertia at once, while the

air throughout the length of the tube will continue to move, but with a diminished velocity. The particles nearest the seat of pressure will be more rapidly impeded, while those more remote will suffer only a gradual retardation. At the instant when the entire column is at a stand still, and possessed of an equilibrium of forces, then there will be a maximum of pressure at the closed end, with an arithmetical diminution of pressure toward the inlet. There will be a gradation of pressures along the sides of the tube, the same as the compression of air throughout its volume.

Now it is evident, if the air were not confined by the walls of the tube, that a lateral movement would take place in the direction of the strongest pressure, having to overcome the inertia of the air moved. This lateral movement would not only be that in line with the opposing surface, but also that extending in all directions, acting as walls in a degree like the walls of the tube.

The moment the air is intercepted that portion in contact with the surface makes the first move to escape, because of the excess of pressure brought to bear upon it. Then follow in succession portions of air more distant.

In the bird's flight it is an object to prevent this too hasty escape of the compressed air, so that more energy may accumulate and impart its effect. A force of air encountering a rigid or irresistible surface is turned from its initial direction on the slightest increase of pressure; but if the surface be elastic, or to a degree non-resistant, then the first imprint of pressure will be received and held in suspense by the receding parts of the surface until the more distant particles of air have arrived and added their burden of energy.

The density of air varies with its pressure, and its inertia increases with its density, so that when the pressure is augmented it has greater persistency against the tendency to a set movement of escape. Again, the opposing surface may be of such nature in its elementary parts as to obstruct the lateral escape by a species of entanglement of the individual particles of air and their inertias.

Air is possessed of a physical property (that of cohesion) which is of vast worth in its relation to our problem. All varieties of matter are possessed of cohesion in greater or less degree. The different degrees of cohesion give rise to solidity, plasticity and viscosity. All solid or rigid substances, so called, are more or less plastic, and all liquids and gases are more or less viscous. Under the effect of impul-

sive forces, plastic substances are as if solid, and viscous substances are as if plastic.

The air in effect behaves as a solid when subjected to impulsive forces of greatest intensity. A case in point is the explosion of nitroglycerine upon the surface of a solid. The mass is ruptured into fragments as if struck by some heavy weight. After the shock the air relieves itself as rapidly as the friction of cohesion and inertia permit.

Work in its mechanical sense is made up of two factors, namely, mass and velocity. $W = \frac{1}{2} M V^2$ is the most general expression, (W) is the quantity of work, (M) the mass or quantity of matter, and (V) the velocity with which it is moved.

Then it will appear, from all that has been said, that the work of velocity rather than the work of mass is the most advantageous source of relief in the bird's every effort. By whatsoever means he may attain velocity, he secures a corresponding relief of duty, and every move of increased velocity given to his wings in opposition to resistances is likewise an economy of power; an increase of velocity permitting a decrease of area of wing, so affording a relief of work. We must avoid the idea that there is of necessity a certain quantity of work for the bird to do, and that he can by no means avoid the set task.

The bird strikes downward with his wings, and secures a pressure of air, which he manages to retain by defeating its every effort to escape. At first his wings decline to the rear, but owing to their torsion and flexure the increasing pressure of air finds relief only through a circuitous path, and at the completion of the stroke the extremities of the wings are tilted rapidly upward.

The wing is so strangely constructed, and its parts are so well adapted to the uses for which they answer, it may be well to study it in detail. It will be necessary to make a few practical demonstrations in order to do our work understandingly.

Let (a, o) (Fig. 4) be a series of elastic surfaces, b, b', b'', etc., and each one confined by its upper edge so that a force of air from the direction denoted by the arrows will cause the surfaces to revolve and close the passages (c, c', c''). This done, the air is defeated in its movement, and a shock or force of impulse secured.

If, instead of the free movement of the slats, they be supposed to have an elastic resistance against being closed, then a force of air of variable intensity will cause them to open and close in rapid succession.

as the force and resistance alternately hold sway. The slats having weight, their inertia in the to and fro movement would act an important part in timing the vibrations to the degree of pressure, and there would be a rhythmical play of impulses or pulsations.

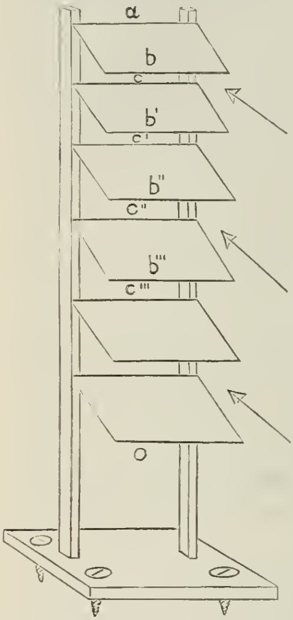

Fig. 4.

Another demonstration may serve to give a more accurate idea of this force of impulse.

Let *a o* (Fig. 5) be a plane surface moving adrift with a wind of any velocity, the plane having the same velocity as the wind. There can be, as yet, no interferenec. Let (*d*) be a fixed point of resistance. The plane will continue to move until it arrives at (*d*), where it will be instantly stopped, thus causing the intercepted column of air to be as quickly stopped and deprived of its moving force. There would be an arrested movement of the air on the two sides of the plane; the difference of the two pressures would be the effective pressure.

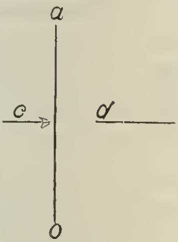

Fig. 5.

So far we have only looked to the force of air as taking effect in one direction; but in the bird's flight there is requisite a lifting and a propelling force.

A force of air after having imparted in one direction a pressure due to its living force may yet impart another pressure in another direction without losing any of its accumulated energy, provided it imparts no movement.

When the bird strikes downward with his wings the resistance of the air acts upward, thus supporting his weight, and also giving him a forward impetus, owing to the escape of the pressure of air to the rear.

The wing, as is well known, is made up of long, stout feathers with quill extremities inserted in cartilaginous sockets of the bird's arm. These quill extremities have muscular attachments, so that the bird has power to revolve the feathers within their sockets. Each feather

is made up of a midrib extending through its entire length, the quill or socket extremity of which is tubular, rigid and larger than the balance. It is translucent, horny and tough, well calculated for strength and at the same time lightness. The balance of the midrib is pithy and not so rigid. It grows smaller and much lighter toward the point. Radiating from both sides are feather branchlets and sub-branchlets, which, from their close proximity, give the feather a smooth surface-like appearance.

Supposing the bird's wing out-stretched, with the quill feathers pointing to the rear. The branchlets that radiate inwardly toward the bird and *underlap* form a more extensive surface than the banchlets of the opposite side of the feather, which *overlap*. The lapping may be of position, without contact, the bird having power to adjust the feathers.

Supposing the surfaces to be properly adjusted, then, as he strikes downward with his wings, a force of air taking effect upon the underlapping surfaces, causes them to collide with the overlapping surfaces, thus abruptly stopping the initial movement of air and its initial energy. This momentary shock or impulse given to the wings serves as a foot-hold to sustain the bird against the tendency of gravity, and as the strokes are made in rapid succession, he may be said to walk upon the air.

The compressed air may seek relief by passing along the surfaces of the wings and escaping from their extreme margins, after having yielded its store of energy first to sustain and then to propel. Or it may find more ready relief by penetrating the network structure of the feathers.

Of these two channels of escape the latter one is sought by those particles of air that come immediately in contact with the wing-surface at the instant of the shock, and are the most active and are possessed of the greatest energy. If permitted, they would destroy the vantage-ground by generating a radial movement of escape. Hence the strangely beautiful structure of the feather.

The pressure of air is first caught, directed and manipulated by the branchlets, and then by the sub-branchlets, when it is allowed to escape from its entanglement. These sub-branchlets radiate from the branchlets, the latter being as midribs. These secondary midribs are discoid in section, and have a tough horny exterior with a pithy centre. See Fig. 6.

b, b', b'', etc., are the branchlets or secondary midribs, on each side of which are the sub-branchlets c, c', c'', etc., overlapping, and d, d', d'' underlapping. The former are longer and more prominent, and the latter form a much less areal surface. These sub-branchlets have their lateral borders barbed with little hooks or loops that enable the over- and underlapping surfaces to cling together, and they hold with a persistent grasp so long as the surfaces are in contact.

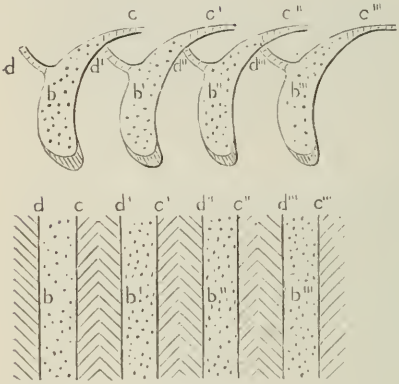

Fig. 6.

From the character of these different parts of the feather and their arrangement with reference to one another it is evident that the elastic force of air taking effect upon them causes a rhythmical vibration, each and every part securing a series of rapid pulsations.

An average feather has about one thousand branchlets, and one million five hundred thousand sub-branchlets, each one of which lends a helping hand to lighten the bird's task. In proportion as there is a greater percussion, the rhythmical pulsations are more frequent and stronger. In the flight of the pigeon and of some other birds this rhythm gives rise to a musical whiz.

So far we have only mentioned the bird proper, and there are almost an infinity of creatures of flight, and one of the most humble is the bat.

The bat, in lifting his wings, raises the front margins more rapidly than the balance of the wing, so that a downward turned fold is given to the thin elastic membrane. At the completion of this fold or loop he thrusts his wing downward, so that the body of air is brought to impinge upon its convexity, suddenly reversing it and bagging the force of air.

I tried the following simple experiment to demonstrate the character of this force. The arm (c d) (Fig. 7) is made of dry hickory and tapers toward d. (b d) is a ring or hoop firmly attached to (c d.) (c d) is mortised with glue to a thick and heavy block of wood. The hoop is covered with a loosely fitting membrane, or paper.

To make the experiment it is only necessary to pull the spring (*c d*) in a direction normally to the surface of the membrane, and then "let fly." The sudden impact of air against the surfaces of the membrane gives two loud reports, the second or reactionary stroke being louder and consequently the stronger. Stout paper may be easily burst by the impact, the report being like that of a pistol.

The experiment may be varied by substituting wire gauze for the membrane and suspending tissue paper before and at a distance from the hoop. The gauze offers little resistance to the movement of the spring, but is greatly impeded by the thin paper on account of its imperviousness to air. In this case also a loud report is given, which is proof of great concussion of the air.

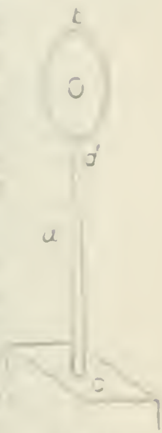

Fig. 7.

It may be proper to say something of the little fly. How does he accomplish such wondrous bodily movements, his wings being so light and delicately slender? It cannot be discovered that he has much power of muscle, and yet it does seem that he must needs make considerable effort to overcome the inertia of his weight in his rapid starts from rest.

It has been shown that in proportion as the air is acted upon more quickly does it exercise greater resistance, and that in proportion as any winged creature strikes the air more quickly, may his wings be smaller and the actual work lessened in great degree. So we may conclude that the fly profits by the rapid movement of his wings. And that such is the case we may be further convinced by regarding the musical note sounded by his wings. Knowing that they must move with great velocity, we must conclude that they do not move normally against the air, but rather at an angle, cutting the air upward, downward and laterally, the torsion of the wings and their delicate elasticity regulating their angular inclinations, *and they are as if smaller under increased velocities.* His wings are so exceedingly light that he needs make but little effort against their inertia. Besides, if they move through curvilinear paths without undergoing accelerated and retarded movements, then is the inertia continuous and self-sustaining.

In conclusion we may say that flight, whether of the bird, the bat or the fly, is not the herculean task of our wonted belief, but rather one of pleasurable ease, unaccompanied by the impediments that clog the way of the creatures that plod the earth.

ON THE MODERN DEVELOPMENT OF FARADAY'S CONCEPTION OF ELECTRICITY.

By Professor Helmholtz.

Abstract of the Faraday Lecture read before the Chemical Society, England, April 5th, 1881.

The majority of Faraday's own researches were connected, directly or indirectly, with questions regarding the nature of electricity, and his most important and most renowned discoveries lay in this field. The facts which he has found are universally known. Nevertheless, the fundamental conceptions by which Faraday has been led to these much-admired discoveries have not been received with much consideration. His principal aim was to express in his new conceptions only facts, with the least possible use of hypothetical substances and forces. This was really a progress in general scientific method, destined to purify science from the last remnants of metaphysics. Now that the mathematical interpretations of Faraday's conceptions regarding the nature of electric and magnetic force has been given by Clerk Maxwell, we see how great a degree of exactness and precision was really hidden behind his words, which to his contemporaries appeared so vague or obscure; and it is astonishing in the highest to see what a large number of general theories the methodical deduction of which requires the highest powers of mathematical analysis, he has found by a kind of intuition, with the security of instinct, without the help of a single mathematical formula.

The electrical researches of Faraday, although embracing a great number of apparently minute and disconnected questions, all of which he has treated with the same careful attention and conscientiousness, are really always aiming at two fundamental problems of natural philosophy, the one more regarding the nature of physical forces, or of forces working at a distance; the other, in the same way, regarding chemical forces, or those which act from molecule to molecule, and the relation between these and the first.

The great fundamental problem which Faraday called up anew for discussion was the existence of forces working directly at a distance without any intervening medium. During the last and the beginning

of the present century, the model after the likeness of which nearly all physical theories had been formed was the force of gravitation acting between the sun, the planets and their satellites. It is known how, with much caution and even reluctance, Sir Isaac Newton himself proposed his grand hypothesis, which was destined to become the first great and imposing example, illustrating the power of true scientific method.

But then came Oerstedt's discovery of the motions of magnets, under the influence of electric currents. The force acting in these phenomena had a new and very singular character. It seemed as if it would drive a single isolated pole of a magnet in a circle around the wire conducting the current, on and on without end, never coming to rest. Faraday saw that a motion of this kind could not be produced by any force of attraction or repulsion, working from point to point. If the current is able to increase the velocity of the magnet, the magnet must react on the current. So he made the experiment, and discovered induced currents; he traced them out through all the various conditions under which they ought to appear. He concluded that somewhere in a part of the space traversed by magnetic force, there exists a peculiar state of tension, and that every change of this tension produces electromotive force. This unknown hypothetical state he called provisionally the electrotonic state, and he was occupied for years and years in finding out what was this electrotonic state. He discovered at first, in 1838, the dielectric polarization of electric insulators, subject to electric forces. Such bodies show, under the influence of electric forces, phenomena perfectly analogous to those exhibited by soft iron under the influence of the magnetic force. Eleven years later, in 1849, he was able to demonstrate that all ponderable matter is magnetized under the influence of sufficiently intense magnetic force, and at the same time he discovered the phenomena of diamagnetism, which indicated that even space, devoid of all ponderable matter, is magnetizable; and now with quite a wonderful sagacity and intellectual precision, Faraday performed in his brain the work of a great mathematician without using a single mathematical formula. He saw, with his mind's eye, that by these systems of tensions and pressures produced by the dielectric and magnetic polarization of space which surrounds electrified bodies, magnets or wires conducting electric currents, all the phenomena of electro-static, magnetic, electro-magnetic attraction, repulsion and induction could be explained, with-

out recurring at all to forces acting directly at a distance. This was the part of his path where so few could follow him; perhaps a Clerk Maxwell, a second man of the same power and independence of intellect, was necessary to reconstruct in the normal methods of science the great building, the plan of which Faraday had conceived in his mind and attempted to make visible to his contemporaries.

Nevertheless the adherents of direct action at a distance have not yet ceased to search for solutions of the electro-magnetic problem. The present development of science, however, shows, as I think, a state of things very favorable to the hope that Faraday's fundamental conceptions may in the immediate future receive general assent. His theory, indeed, is the only existing one which is at the same time in perfect harmony with the facts observed, and which at least does not lead into any contradiction against the general axioms of dynamics.

It is not at all necessary to accept any definite opinion about the ultimate nature of the agent which we call electricity.

Faraday himself avoided as much as he could giving any affirmative assertion regarding this problem, although he did not conceal his disinclination to believe in the existence of two opposite electric fluids.

For our own discussion of the electro-chemical phenomena, to which we shall turn now, I beg permission to use the language of the old dualistic theory, because we shall have to speak principally on relations of quantity.

I now turn to the second fundamental problem aimed at by Faraday, the connection between electric and chemical force. Already, before Faraday went to work, an elaborate electro-chemical theory had been established by the renowned Swedish chemist, Berzelius, which formed the connecting-link of the great work of his life, the systematization of the chemical knowledge of his time. His starting point was the series into which Volta had arranged the metals according to the electric tension which they exhibit after contact with each other. A fundamental point which Faraday's experiment contradicted was the supposition that the quantity of electricity collected in each atom was dependent on their mutual electro-chemical differences, which he considered as the cause of their apparently greater chemical affinity. But although the fundamental conceptions of Berzelius' theory have been forsaken, chemists have not ceased to speak of positive and negative constituents of a compound body. Nobody

can overlook that such a contrast of qualities, as was expressed in Berzelius' theory, really exists, well developed at the extremities, less evident in the middle terms of the series, playing an important part in all chemical actions, although often subordinated to other influences.

When Faraday began to study the phenomena of decomposition by the galvanic current, which, of course, were considered by Berzelius as one of the firmest supports of his theory, he put a very simple question; the first question, indeed, which every chemist speculating about electrolysis ought to have answered. He asked, What is the quantity of electrolytic decomposition if the same quantity of electricity is sent through several electrolytic cells? By this investigation he discovered that most important law, generally known under his name, but called by him the law of definite electrolytic action.

Faraday concluded from his experiments that a definite quantity of electricity cannot pass a voltametric cell containing acidulated water between electrodes of platinum without setting free at the negative electrode a corresponding definite amount of hydrogen, and at the positive electrode the equivalent quantity of oxygen, one atom of oxygen for every pair of atoms of hydrogen. If instead of hydrogen any other element capable of substituting hydrogen is separated from the electrolyte, this is done also in a quantity exactly equivalent to the quantity of hydrogen which would have been evolved by the same electric current.

Since that time our experimental methods and our knowledge of the laws of electrical phenomena have made enormous progress, and a great many obstacles have now been removed which entangled every one of Faraday's steps, and obliged him to fight with the confused ideas and ill-applied theoretical conceptions of some of his cotemporaries. We need not hesitate to say that the more experimental methods were refined, the more the exactness and generality of Faraday's law was confirmed.

In the beginning Berzelius and the adherents of Volta's original theory of galvanism, based on the effects of metallic contact, raised many objections against Faraday's law. By the combination of Nobili's astatic pairs of magnetic needles with Schweigger's multiplicator, a coil of copper wire with numerous circumvolutions, galvanometers became so delicate that the electro-chemical equivalent of the smaller currents they indicated was imperceptible for all chemical methods. With the newest galvanometers you can very well observe currents which would

want to last a century before decomposing one milligram of water, the smallest quantity which is usually weighed on chemical balances. You see that if such a current lasts only some seconds or some minutes, there is not the slightest hope to discover its products of decomposition by chemical analysis. And even if it should last a long time the feeble quantities of hydrogen collected at the negative electrode can vanish, because they combine with the traces of atmospheric oxygen absorbed by the liquid. Under such conditions a feeble current may continue as long as you like without producing any visible trace of electrolysis, even not of galvanic polarization, the appearance of which can be used as an indication of previous electrolysis. Galvanic polarization, as you know, is an altered state of the metallic plates which have been used as electrodes during the decomposition of an electrolyte. Polarized electrodes, when connected by a galvanometer, give a current which they did not give before being polarized. By this current the plates are discharged again and returned to their original state of equality.

This depolarizing current is indeed a most delicate means of discovering previous decomposition. I have really ascertained that under favorable conditions one can observe the polarization produced during some seconds by a current which decomposes one milligram of water in a century.

Products of decomposition cannot appear at the electrodes without motions of the constituent molecules of the electrolyte throughout the whole length of the liquid. This subject has been studied very carefully and for a great number of liquids, by Prof. Hittorff, of Münster, and Prof. G. Wiedemann, of Leipsic.

Prof. F. Kohlrausch, of Würzburg, has brought to light the very important fact that in diluted solutions of salts, including hydrates of acids and hydrates of caustic alkalies, every atom under the influence of currents of the same density moves on with its own peculiar velocity, independently of other atoms moving at the same time in the same or in opposite directions. The total amount of chemical motion in every section of the fluid is represented by the sum of the equivalents of the cation gone forwards and of the anion gone backwards, in the same way as in the dualistic theory of electricity, and the total amount of electricity flowing through a section of the conductor corresponds to the sum of positive electricity going forwards and negative electricity going backwards.

This established, Faraday's law tells us that through each section of an electrolytic conductor we have always equivalent electrical and chemical motion. The same definite quantity of either positive or negative electricity moves always with each univalent ion, or with every unit of affinity of a multivalent ion, and accompanies it during all its motions through the interior of the electrolytic fluid. This we may call the electric charge of the atom.

Now the most startling result, perhaps, of Faraday's law is this. If we accept the hypothesis that the elementary substances are composed of atoms we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity. As long as it moves about on the electrolytic liquid each atom remains united with its electric equivalent or equivalents. At the surface of the electrodes decomposition can take place if there is sufficient electromotive power, and then the atoms give off their electric charges and become electrically neutral.

Now arises the question, Are all these relations between electricity and chemical combination limited to that class of bodies which we know as electrolytes? In order to produce a current of sufficient strength to collect enough of the products of decomposition without producing too much heat in the electrolyte, the substance which we try to decompose ought not to have too much resistance against the current. But this resistance may be very great, and the motion of the ions may be very slow, so slow indeed that we should need to allow it to go on for hundreds of years before we should be able to collect even traces of the products of decomposition; nevertheless all the essential attributes of the process of electrolysis could subsist. If you connect an electrified conductor with one of the electrodes of a cell filled with oil of turpentine, the other with the earth, you will find that the electricity of the conductor is discharged unmistakably more rapidly through the oil of turpentine than if you take it away and fill the cell only with air.

Also in this case we may observe polarization of the electrodes as a symptom of previous electrolysis. Another sign of electrolytic conduction is that liquids brought between two different metals produce an electromotive force. This is never done by metals of equal temperature, or other conductors which, like metals, let electricity pass without being decomposed.

The same effect is also observed even with a great many rigid bodies,

although we have very few solid bodies which allow us to observe this electrolytic conduction with the galvanometer, and even these only at temperatures near to their melting-point. It is nearly impossible to shelter the quadrants of a delicate electrometer against being charged by the insulating bodies by which they are supported.

In all the cases which I have quoted one might suspect that traces of humidity absorbed by the substance or adhering to their surface were the electrolytes. I show you, therefore, this little Daniell's cell, in which the porous septum has been substituted by a thin stratum of glass. Externally all is symmetrical at both poles; there is nothing in contact with the air but a closed surface of glass, through which two wires of platinum penetrate. The whole charges the electrometer exactly like a Daniell's cell of very great resistance, and this it would not do if the septum of glass did not behave like an electrolyte. All these facts show that electrolytic conduction is not at all limited to solutions of acids or salts.

Hitherto we have studied the motions of ponderable matter as well as of electricity, going on in an electrolyte. Let us study now the forces which are able to produce these motions. It has always appeared somewhat startling to everybody who knows the mighty power of chemical forces, the enormous quantity of heat and of mechanical work which they are able to produce, and who compares with it the exceedingly small electric attraction which the poles of a battery of two Daniell's cells show. Nevertheless this little apparatus is able to decompose water.

The quantity of electricity which can be conveyed by a very small quantity of hydrogen, when measured by its electrostatic forces, is exceedingly great. Faraday saw this, and has endeavored in various ways to give at least an approximate determination. The most powerful batteries of Leyden jars, discharged through a voltameter, give scarcely any visible traces of gases. At present we can give definite numbers. The result is that the electricity of 1 m.grm. of water, separated and communicated to two balls, 1 kilometre distant, would produce an attraction between them, equal to the weight of 25,000 kilos.

The total force exerted by the attraction of an electrified body upon another charged with opposite electricity is always proportional to the quantity of electricity contained in the attracting as on the attracted body, and therefore even the feeble electric tension of two Daniell's

elements, acting through an electrolytic cell upon the enormous quantities of electricity with which the constituent ions of water are charged, is mighty enough to separate these elements and to keep them separated.

We now turn to investigate what motions of the ponderable molecules require the action of these forces. Let us begin with the case where the conducting liquid is surrounded everywhere by insulating bodies. Then no electricity can enter, none can go out through its surface, but positive electricity can be driven to one side, negative to the other, by the attracting and repelling forces of external electrified bodies. This process going on as well in every metallic conductor is called "electrostatic induction." Liquid conductors behave quite like metals under these conditions. Prof. Wüllner has proved that even our best insulators, exposed to electric forces for a long time, are charged at last quite in the same way as metals would be charged in an instant. There can be no doubt that even electromotive forces going down to less than $\frac{1}{100}$ Daniell produce perfect electrical equilibrium in the interior of an electrolytic liquid.

Another somewhat modified instance of the same effects is afforded by a voltametric cell containing two electrodes of platinum, which are connected with a Daniell's cell, the electromotive force of which is insufficient to decompose the electrolyte. Under this condition the ions carried to the electrodes cannot give off their electric charges. The whole apparatus behaves, as was first accentuated by Sir W. Thomson, like a condenser of enormous capacity.

Observing the polarizing and depolarizing currents in a cell containing two electrodes of platinum, hermetically sealed and freed of all air, we can observe these phenomena with the most feeble electromotive forces of $\frac{1}{1000}$ Daniell, and I found that down to this limit the capacity of the platinum surfaces proved to be constant. By taking greater surfaces of platinum, I suppose it will be possible to reach a limit much lower than that. If any chemical force existed besides that of the electrical charges which could bind all the pairs of opposite ions together, and require any amount of work to be vanquished, an inferior limit to the electromotive forces ought to exist, which forces are able to attract the atoms to the electrodes and to charge these as condensers. No phenomenon indicating such a limit has as yet been discovered, and we must conclude, therefore, that no

other force resists the motions of the ions through the interior of the liquid than the mutual attractions of their electric charges.

On the contrary, as soon as an ion is to be separated from its electrical charge we find that the electrical forces of the battery meet with a powerful resistance, the overpowering of which requires a good deal of work to be done. Usually the ions, losing their electric charges, are separated at the same time from the liquid; some of them are evolved as gases, others are deposited as rigid strata on the surface of the electrodes, like galvanoplastic copper. But the union of two constituents having powerful affinity to form a chemical compound, as you know very well, produces always a great amount of heat, and heat is equivalent to work. On the contrary, decomposition of the compound substances requires work, because it restores the energy of the chemical forces, which has been spent by the act of combination.

Metals uniting with oxygen or halogens produce heat in the same way, some of them, like potassium, sodium, zinc, even more heat than an equivalent quantity of hydrogen; less oxidizable metals, like copper, silver, platinum, less. We find, therefore, that heat is generated when zinc drives copper out of its combination with the compound halogen of sulphuric acid, as in the case in a Daniell's cell.

If a galvanic current passes through any conductor, a metallic wire, or an electrolytic fluid, it evolves heat. Mr. Prescott Joule was the first who proved experimentally that if no other work is done by the current, the total amount of heat evolved in galvanic circuit during a certain time is exactly equal to that which ought to have been generated by the chemical actions which have been performed during that time. But this heat is not evolved at the surface of the electrodes, where these chemical actions take place, but is evolved in all the parts of the circuit, proportionally to the galvanic resistance of every part. From this it is evident that the heat evolved is an immediate effect, not of the chemical action, but of the galvanic current, and that the chemical work of the battery has been spent in producing only the electric action.

If we apply Faraday's law, a definite amount of electricity passing through the circuit corresponds to a definite amount of chemical decomposition going on in every electrolytic cell of the same circuit. According to the theory of electricity, the work done by such a definite quantity of electricity which passes, producing a current, is proportionate to the electromotive force acting between both ends of the

conductor. You see, therefore, that the electromotive force of a galvanic circuit must be, and is indeed, proportionate to the heat generated by the sum of all the chemical actions going on in all the electrolytic cells during the passage of the same quantity of electricity. In cells of the galvanic battery chemical forces are brought into action able to produce work; in cells in which decomposition is occurring work must be done against opposing chemical forces; the rest of the work done appears as heat evolved by the current, as far as it is not used up to produce motions of magnets or other equivalents of work.

Hitherto we have supposed that the ion with its electric charge is separated from the fluid. But the ponderable atoms can give off their electricity to the electrode, and remain in the liquid, being now electrically neutral. This makes almost no difference in the value of the electromotive force. For instance, if chlorine is separated at the anode, it will remain at first absorbed by the liquid; if the solution becomes saturated, or if we make a vacuum over the liquid, the gas will rise in bubbles. The electromotive force remains unaltered. The same may be observed with all the other gases. You see in this case that the change of electrically negative chlorine into neutral chlorine is the process which requires so great an amount of work, even if the ponderable matter of the atoms remains where it was.

The more the surface of the positive electrode is covered with negative atoms of the anion, and the negative with the positive ones of the cation, the more the attracting force of the electrodes exerted upon the ions of the liquid is diminished by this second stratum of opposite electricity covering them. On the contrary, the force with which the positive electricity of an atom of hydrogen is attracted towards the negatively charged metal increases in proportion as more negative electricity collects before it on the metal, and the more negative electricity collects behind it in the fluid.

Such is the mechanism by which electric force is concentrated and increased in its intensity to such a degree that it becomes able to overpower the mightiest chemical affinities we know of. If this can be done by a polarized surface, acting like a condenser, charged by a very moderate electromotive force, can the attractions between the enormous electric charges of anions and cations play an unimportant and indifferent part in chemical affinity?

You see, therefore, if we use the language of the dualistic theory and treat positive and negative electricities as two substances, the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom with different force. Potassium, sodium, zinc, must have strong attraction to a positive charge; oxygen, chlorine, bromine to a negative charge.

Faraday very often recurs to this to express his conviction that the forces termed chemical affinity and electricity are one and the same. I have endeavored to give you a survey of the facts in their mutual connection, avoiding, as far as possible, introducing other hypotheses, except the atomic theory of modern chemistry. I think the facts leave no doubt that the very mightiest among the chemical forces are of electric origin. The atoms cling to their electric charges and the opposite electric charges cling to the atoms. But I don't suppose that other molecular forces are excluded, working directly from atom to atom. Several of our leading chemists have begun lately to distinguish two classes of compounds, molecular aggregates and typical compounds. The latter are united by atomic affinities, the former not. Electrolytes belong to the latter class.

If we conclude from the facts that every unit of affinity of every atom is charged always with one equivalent either of positive or of negative electricity, they can form compounds, being electrically neutral, only if every unit charged positively unites under the influence of a mighty electric attraction with another unit charged negatively. You see that this ought to produce compounds in which every unit of affinity of every atom is connected with one and only with one other unit of another atom. This is, as you will see immediately, indeed, the modern chemical theory of quantivalence, comprising all the saturated compounds. The fact that even elementary substances, with few exceptions, have molecules composed of two atoms, makes it probable that even in these cases electric neutralization is produced by the combination of two atoms, each charged with its electric equivalent, not by neutralization of every single unit of affinity.

But I abstain from entering into mere specialties, as, for instance, the question of unsaturated compounds; perhaps I have gone already too far. I would not have dared to do it if I did not feel myself sheltered by the authority of that great man who was guided by a never-erring instinct of truth. I thought that the best I could do for

his memory was to recall to the minds of the men, by the energy and and intelligence of whom chemistry has undergone its modern astonishing development, what important treasures of knowledge lie still hidden in the works of that wonderful genius. I am not sufficiently acquainted with chemistry to be confident that I have given the right interpretation, that interpretation which Faraday himself would have given perhaps, if he had known the law of chemical quantivalence, if he had had the experimental means of ascertaining how large the extent, how unexceptional the accuracy of his law really is; and if he had known the precise formulation of the law of energy applied to chemical work, and of the laws which determine the distribution of electric forces in space as well as in ponderable bodies transmitting electric current or forming condensers. I shall consider my work of to-day well rewarded if I have succeeded in kindling anew the interest of chemists for the electro-chemical part of their Science.

RECENT ADVANCES IN PHOTOGRAPHY, NEGATIVE AND POSITIVE.

By John Carbutt.

Abstract of a Lecture delivered before the Franklin Institute, April 25, 1881.

Among the numerous civilizing and educational agencies of the nineteenth century that have been and are now benefitting mankind stand pre-eminent the Steam Engine, Electricity and Photography. Upon the two former I need not dwell before an audience of the Franklin Institute.

It may, however, be interesting, and perhaps to some instructive, to briefly trace the history of photography, before proceeding to the description and illustration of modern photography, negative and positive, this being the main object for which I have the honor to appear before you this evening.

Photography means literally writing by means of light, and is of much older origin than many suppose. The eminent Swedish chemist, Scheele, was the first to notice the decomposing action of light on compounds containing silver, and in 1777 obtained the first photograph of the solar spectrum.

The first attempt to render the action of light available in the repro-

duction of drawings was made about 1802 by Wedgwood and Davy, who, while they succeeded in obtaining images by aid of the sun's rays, were unable to fix or render unalterable those parts of the sensitive surface not acted upon by the light.

It was not, however, until the discovery by Sir John Herschel of the power of hyposulphite of soda to dissolve the salts of silver that are insoluble in water, that any real progress was made in this art. The discoveries of Daguerre, of France, and Talbot, of England, were made known to the world in 1839; for some years previous both had been experimenting to produce photogenic drawings by the aid of light and the camera obscura (the invention of Baptista Porto in the sixteenth century), Daguerre on metal and Talbot on paper.

Daguerre took a polished silver plate, made sensitive to light by exposing it to the vapor of iodine, and after exposure in the camera developed the image by the vapor of mercury.

Talbot used paper, coating the surface with a solution of iodide of potassium in water, and when dry again coating with an aqueous solution of nitrate of silver, drying and exposing in the camera, developing the image with gallo-nitrate of silver.

The Daguerreotype process, as given to the world, was but in a crude form, the sensitiveness being of a low degree, requiring 15 to 20 minutes' exposure in sunlight to obtain an impression, and the image not permanent; nevertheless, the results at that time were considered marvelous. On the process becoming known, men of science took up the study of Daguerreotyping, and speedily improved upon the results obtained by Daguerre. To M. Godard, of London, is due the credit of increasing the sensitiveness of the plates by using the vapor of bromine in addition to iodine, as employed by Daguerre, and to M. Fizeau, of Paris, the rendering of the impression permanent by giving the plate a slight gilding of gold. The Daguerreotype, while one of the most beautiful pictures ever produced by photography, owing to its reversed image and want of duplicating power, received its death-blow on the advent of Archer's collodion process in 1851, and was practiced for only a few years after that time.

The Calotype process of Fox Talbot, or Talbotype as it is generally named, was never a favorite, on account of the difficulty of getting rid of the effect of the grain or fibre of the paper on which the negative was made; but Le Gray, of Paris, removed this objection in a great measure by his improvement of waxing the paper negative.

This process was succeeded by the albumen process of Niepce de Saint Victor, albumen being used as the vehicle to hold the sensitive salts of silver on glass plates, as suggested by Sir John Herschel; although a slow process, it yielded negatives of fine quality, also positives, and is still extensively used in France in the production of transparencies for the lantern. Messrs. Langenheim Bros. and Francis Schreiber, of Philadelphia, as well as others in America, were successful workers of this process.

In 1851 F. Scott Archer, of London, as previously stated, gave to the world his collodion process, which, with slight modifications, has been used as he gave it for nearly thirty years, and is called by the profession the *wet process*. Its use is confined chiefly to the building or gallery of the photographer, the great difficulties involved in transporting the chemicals and "dark room" apparatus in a serviceable condition for out-door photographing restricting its use mainly to professionals. Since the advent of collodion constant effort has been made to prepare the sensitive plates in a dry state, with collodion or collodion and albumen combined, and with considerable success by both professional and amateur photographers; yet the most successful in preparing these plates felt seriously at times that a most essential quality—rapidity or sensitiveness—was lacking, as they required an exposure of from four to ten times as long as wet collodion plates. It was not until another vehicle for holding the sensitive salts of silver was found, that a rapidity of action equal to and exceeding the most sensitive wet collodion plate was attained, and dry-plate photography became practicable to the professional photographer.

In the new method gelatine is the material with which the sensitive salts of silver are combined, and plates are prepared of the most exalted sensitiveness. This process is not yet ten years old. To Dr. R. L. Maddox, of London, is due the credit of first publishing a formula for their preparation. For six or seven years very little notice was taken of it, but for the past two years the photographic world, or that part of it on the other side of the Atlantic, has become enthusiastic over it, and now the fever has reached American photographers, both amateur and professional.

This gelatino-bromide process, as it is generally called, is certainly a wonderful advance in the art science of photography, enabling impressions in the camera to be obtained in from one-sixth to one-tenth the time of the most rapid collodion plate hitherto employed. The pres-

paration of these "gelatino-bromide" plates is in brief as follows: To a solution of fine gelatine in water is added bromide of potassium or ammonium; in another portion of water is dissolved nitrate of silver; in a room lighted through dark ruby glass, the solution of silver is added by degrees to the bromide and gelatine, and well stirred. It must be kept in a fluid condition for some hours, at a moderate temperature, and, where great sensitiveness is required, for one to four days, or the time may be confined to an hour or two by using a higher degree of heat. It now remains only to free it from the nitrates of potassium or ammonium, formed during the making of the emulsion, which is done by pouring the emulsion into a porcelain dish and allowing it to set to a stiff jelly, after which it is broken up, washed in several changes of cold water, drained and remelted for use, and the plates are coated and dried, and are then ready for use, and may be kept any length of time. These plates are being rapidly adopted by the profession, not only for viewing, but for the regular practice of photography in the studio, as they require no preparation, but are ready at any and all times when the operator is prepared to make the exposure. For scientific explorations they are of great value. I have secured a few prints from negatives taken by the photographer on the Howgate Expedition to the Arctic regions, as well as some by professional and amateur photographers, on gelatine plates made in this city, and known as the "Keystone" brand. To convey to you some idea of the progress made in shortening the time of exposure in the camera, one of the pictures to be shown on the screen was said to have been taken in the 150th part of a second. Compare this with the fifteen minutes exposure given to the Daguerreotype spoken of, and you will find it to be one 270,000th part of that time. It would not be fair to claim for gelatine dry plates all the credit of securing these wonderful results; the perfection to which our optical instruments have been brought has much to do with it, and I have here to show you the most recent improvements in dry-plate apparatus, made by the Scovill Manufacturing Co., New York.

I will now exhibit to you a few slides made on gelatine plates from gelatine negatives, and then proceed with the remainder of my subject —positive photography and its improvements.

Reproducing the image of the negative on paper was first confined to Talbot's Calotype process. Plain chloride of silver paper was then used both for direct printing and gallic acid development and some

of the latter prints have stood the test of time better than any since made with the silver salts. Albumen paper was introduced about 1854—that is, paper coated with a layer of albumen containing a certain amount of chloride of ammonium or sodium and dried. It is sensitized by floating it on a solution of nitrate of silver. After drying it is printed under the negative, toned by passing it through a weak solution of chloride of gold, then into a solution of hyposulphite of soda, technically called the fixing-bath. After a thorough washing the picture is finished, so far as any chemical treatment is needed. This has been, and is yet, the staple process by which the greater bulk of positive impressions is produced. It was early discovered that the permanence of the prints was not assured, and various attempts have been made to replace the albumen print by one of a more durable nature. The process that at one time seemed destined to supplant it was the carbon process, which was adopted by some few photographers, to the exclusion of silver printing; but, after a thorough trial, it had to be abandoned for the old process. The carbon process finds but limited application in America, while in England it is made use of to a large extent in the reproduction of works of art.

The next process to attract public attention was the platinum process of W. Willis, Jr., of London, but now of New York. In this process it was aimed to replace the silver salt by the less oxidizable metal platinum. At first, while the process gave very beautiful results, it required chemical knowledge on the part of the operator, and so much care that it was practically valueless. About two years ago Mr. Willis discovered a method by which all these objectionable features were removed—the process much simplified and reliable results obtained with ordinary care. This process, in its improved form, I shall now describe, and develop some prints before you.

Only four chemicals are made use of; they are ferric oxalate, potassic oxalate, potassic chloro-platinate and hydrochloric acid. One ounce of the ferric oxalate solution is taken, and from 40 to 60 grains of the potassic chloro-platinate is dissolved therein. This is the sensitizing solution, which is applied to the surface of the paper to be used, by means of a pad of flannel. The paper is allowed to become surface dry, and is then perfectly dried by the aid of heat. The exposure to light is made in the usual manner, either under the negative, or when an enlargement is desired, by the use of the solar camera or electric light. The exposed print is developed in or on a hot solu-

tion of potassic oxalate, and is then immersed in a weak bath of hydrochloric acid to dissolve out the iron salt left in the paper, and finally washed in three or four baths of plain water.

I will now conclude by developing some prints sent to me by Mr. Willis. You will observe a faint image on this sheet of paper, produced by the action of light; it is a picture in ferrous oxalate, the iron salt only being reduced from ferric oxalate to ferrous oxalate, the platinum salt being in intimate mixture only. Now if to a solution of ferrous oxalate in oxalate of potassa, a solution of platinum chloride be added, you will instantly see a decomposition of the platinum salt in the shape of a fine black powder; and this is what will take place as I now pass the print through this hot solution of oxalate of potash. You see, where the action of the light has been strongest, the greatest quantity of platinum is precipitated; this is caused by the dissolving of the ferrous oxalate by the potassic oxalate solution, when the two combined instantly reduce the platinum salt in situ, forming an image in platinum black, known as spongy platinum. Prints by this process have been subjected to the severest chemical tests without having their permanence affected in the least. I now pass the print into this water, acidified with hydrochloric acid, which will dissolve out the iron salt not acted on by the light—two or three changes in fresh water completing the process.

Another improvement in photographic printing is now being developed. It is the application of gelatino-bromide of silver to paper for enlargements and contact printing, the image being developed with ferrous oxalate. Gelatino-chloride of silver has also been used for contact printing in place of albumen paper. As yet these improvements have not been brought into use in America, but I am safe in saying there is no doubt they soon will be, as American photographers are too enterprising to let any improvement having merit lie idle.

Prophets without Honor.—The old proverb is illustrated by the inventors of thermometers. In England they use Fahrenheit's thermometer, the invention of a German. In Germany the thermometer of Reaumur, a Frenchman, is still the most common. In France and in many other countries the Centigrade thermometer, which was invented by the Swede Celsius, is universally adopted.—*Verkehrszeitung.* C.

MODERN CYANOTYPE PRINTING.

By Dr. J. M. Eder.

Although Herschel showed in 1842, by means of ferrocitrate and ferrocyanide of potassium, how positive cyanotypes were to be produced (blue lines upon a white ground), the process was one that has rarely been adopted in practice. It is so exceedingly difficult to obtain clear prints; the ground is always more or less blue.

Pellet was the first to show results of a faultless character, and in the last International Exhibition held at Paris, in 1878, some very fine positive cyanotypes were to be seen. The process, however, by which these were produced was not made known in its entirety. It was simply stated that the employment of slimy, gum-like substances was resorted to.

The details of a most excellent gum-iron process have now been given by Captain Pizzighelli; this method acts thoroughly well, and yields most satisfactory prints. Thirty volumes of a solution of gum arabic (water 5 parts, gum 1 part) are mixed with 8 volumes of an aqueous solution of citrate of iron and ammonia (water 2 parts, double salt 1 part), and to the mixture is added 5 volumes of an aqueous solution of perchloride of iron (water 2 parts, iron 1 part).

The mixture appears limpid at first, but soon grows thicker, and it should be used quickly after mixing it. It is applied to well-sized paper by means of a brush, the paper being dried in the dark.

Any design, drawing or tracing may be employed as negative, and, after printing a few minutes, the development is proceeded with. A solution of ferrocyanide of potassium (water 5 parts, ferrocyanide 1 part) is applied with a brush, and the picture appears almost instantly as a dark blue positive. As soon as every detail has appeared the print is quickly rinsed, and then put into a dish containing dilute hydrochloric acid (water 10, acid 1), when the image becomes clearer and brighter, the ground gets white and the gum-iron film is removed. After further washing the print is dried.

The printing and finishing of impressions proceed very rapidly; in fair weather it takes from one to two hours to carry out the whole process, preparing the paper into the bargain.

The whole secret of success lies in the use of the gum arabic, which forms with the iron salts an almost insoluble combination, covering the paper like a varnish. For this reason the pores of the paper are not filled with coloring matter where no color is wanted. The acid bath removes the varnish-like film of gum, and leaves the clear positive picture in blue behind.—*Photographic News.*

Sugar from Rags.—The manufacture of sugar from old rags is now carried on on a large scale. A German factory is regularly engaged in the business, treating the rags first by sulphuric acid so as convert them into dextrine. The dextrine is bleached, by means of milk of lime, and then submitted to a new sulphuric acid bath stronger than the first, after which, being transformed into crystals of glucose, it can be employed in jellies and confections. The glucose which is obtained by this process can be sold very cheaply, and it resembles chemically that which is derived from grapes. The attention of the German government has been called to the danger, in a hygienic point of view, which may arise from the use of this article.—*Chron. Ind.* C.

The Thermophone.—Mercadier, in his experiments upon radiophony, has succeeded in producing the desired effects by means of lights which are much more feeble than an ordinary gas jet, and even by invisible radiations. He first noticed that when powerful lights were used there was no need of concentrating lenses; it was sufficient to bring the lights as near as possible to the interrupting wheel, limiting the pencil of rays by means of a diaphragm of suitable opening, still nearer to the wheel. He then took a copper disc of two millimetres (·079 in.) thickness and about four centimetres (1·575 in.) diameter, fixed at a few centimetres from the diaphragm, and gradually heated the diaphragm upon the face opposed to the wheel by means of an oxy-hydrogen light. He thus obtained a source of radiations at first invisible, but of which the temperature could be gradually raised to a dull red and to a bright red. In the latter case the sounds produced are very plainly heard, and if the light is extinguished the sounds continue to be audible even after the disc ceases to be visible in the darkness. The experiment can be made without difficulty with receivers of glass or mica, thin and smoked so as to have a true thermophone.—*Comptes Rendus.* C.

Thermal Conductivity of Fluids.—H. F. Weber has experimented upon the thermal conductivity of water, glycerine, alcohol, ether, chloroform, benzine, olive oil, citron oil and various solutions, and he finds that transparent, non-metallic fluids have nearly equal conductive power at equal temperatures.—*Wiedem. Ann.* C.

Dressing for Leather.—A fine, brilliant, elastic dressing for leather, which does not injure shoes, can be made as follows: To three pounds of boiling water add, with continual stirring, a half pound of white wax, an ounce of transparent glue, two ounces of gum senegal, one and a half ounces white soap, and two ounces of brown candy. Finally, add two and a half ounces of alcohol, and, after the whole is cooled, three ounces of fine Frankfort black. The dressing is thinly applied to the leather with a soft brush and after it is dried it is rubbed with a piece of fine pumice stone and polished with a stiff brush.—*Badische Gewerbe-Zeitung.* C.

Accurate Thermometers.—A. Wüllner has been making new investigations of the specific heat of water, and he has found additional reasons for insisting upon the importance of careful thermometric graduation. The customary method of fixing the freezing and boiling points, and dividing the interval regularly, is very inaccurate, and in many of the modern delicate physical investigations it leads to results which are wholly untrustworthy. He asserts that no quicksilver thermometer should be used in physical measurements which has not been carefully graduated throughout the entire length of its scale by comparison with an accurate air thermometer.—*Wiedemann's Annalen.* C.

Artificial Soil.—M. Dudouy, of Saint Ouen, has been very successful in chemical horticulture. In his garden he has cultivated legumes, flowers and trees in parallel rows in three manners: 1. with ordinary manure; 2. with chemical manures in garden soil; 3. with a special compound, which he calls *floral*, in pure sand. The results of the third experiments have been very striking, yielding the earliest, the largest and the most delicate vegetables, as well as the most thrifty and brilliant flowers. The *floral* contains nitrogen, phosphoric acid, potash, magnesia and sulphur in a form so concentrated as to require dilution with twenty thousand times their volume of water. The experiments have been continued for five years with uniform success.—*Les Mondes.* C.

Bursting Power of Ice.—Ed. Hagenbach experimented, during the past severe winter, upon the bursting force exerted in the expansion of water when freezing. Two interesting experiments were made with cast-iron hand grenades. The outer diameter was 15 centimetres (5·9 in.), the inner diameter 12·8 cm. (5·04 in.) The shells were filled with water, closed with a screwed iron plug, and exposed to the cold. Both shells were broken, and a curved thread of ice was projected, by means of an ice column, from the upper surface. One of the plugs was evidently thrown out with great violence, and to such a distance that it could not be found. The curvature in that case was bent upward.—*Wiedemann's Annalen.* C.

Comparative Value of Steam Engines.—Hallauer's recent experiments have led him to the conclusion that the difference between engines of one and two cylinders, in point of economy, is very slight. In ranging from 80 to 8000 horse-power, with revolutions varying from 25 to 90 per minute, the expenditure of steam for a given amount of work remains the same for the same type of motor; the consumptions for two cylinder motors are identical for Woolf and compound, whatever may be the volumes of the cylinders, provided the motors are regulated so as to give the maximum efficiency; the expenditures of steam in motors of one, two and three cylinders, suitably regulated and constructed, are so nearly alike that the choice may be governed in each instance merely by the fitness of the type of the engine for the particular purpose desired.—*Bull. de la Soc. Ind. de Mulhouse.* C.

Franklin Institute.

Hall of the Institute, May 18th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 58 members and 9 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced that at their last meeting 13 persons were elected members of the Institute.

Mr. Cartwright asked and obtained unanimous consent for a suspension of the order of business, to take up the consideration of the

repeal of section 4 of Article III of the By-laws, as recommended by the Board of Managers; also, to alter section 1 of Article I to read as follows:

"The real and personal estates of the Institute, as held upon the 1st day of January, 1881, shall be valued at one hundred thousand dollars, and shall be represented by ten thousand shares of stock of the par value of ten dollars each; and in addition thereto, all sums of money or other property contributed to the Trustees of the Building Fund, for the purposes of the Institute, shall be represented by an additional number of shares of stock, of the same par value of ten dollars each, which may be issued by authority of the Board of Managers."

Upon motion, the Secretary was directed to make the necessary publication of the above.

The following donations to the Library are reported:

Transactions Pennsylvania State Agricultural Society for 1871–2; 1874–5; 1878–1880. From the Society.

Transactions from the Wisconsin State Agricultural Society for 1874–5; 1876–7; 1879–80. From the Society.

Transactions of the Illinois State Agricultural Society for 1859–60; 1872; 1874; 1876; 1877, and 1879.
From the Department of Agriculture.

Annual Report of Operations of the United States Life-saving Service for year ending June 30, 1880.
From the Life-saving Bureau.

Minutes of Proceedings of Institution of Civil Engineers. Vol. 63. London. 1880–1881. From the Institution.

Report of the Board of School Commissioners of Baltimore for 1880. From the Commissioners.

Papers Relating to the Foreign Relations of the United States. 1880. From the Department of State.

Memorial of Joseph Henry.
Smithsonian Contributions to Knowledge. Vol. 23.
Smithsonian Miscellaneous Collections. Vols. 18—21.
From Smithsonian Institution.

Official Army Register for 1881.
From Adjutant-General U. S. A., Washington.

On the Ventilation of Halls of Audience. By Robert Briggs, C.E. Philadelphia. From the Author.

Ninth Annual Report of the Board of Directors of the Zoölogical Society of Philadelphia. 1881. From the Society.

Useful Information Pertaining to the Generation and Use of Steam. From Babcock & Wilcox, N. Y.

Preliminary Report upon the Iron and Steel Industries of the United States. By James M. Swank. From the Author.

Almanaque Nautico para 1881 and 1882.
From Observatory of the Maritime Institute, San Fernando, Spain.

Proceedings American Academy of Arts and Sciences. Vols. 1—8. 1846–1873; New Series, Vols. 1—8 (Part 1) 1873–1881.
From the Academy.

Dr. Robert Grimshaw read an interesting paper on "Air Compressors," and a number of photographs of various machines were projected upon the screen. An abstract of his paper is here appended:

"Air under pressure is employed for the transmission of power, as the principal or auxiliary of various industries, in mines, in quarries, well driving, building bridge piers, in tunneling, in the production of cold, in sugar-making and refining, and various chemical works, the manufacture of iron and steel, for ventilating mines or buildings, for transmitting letters or packages in pipes placed underground or otherwise, in driving street cars, etc. It makes no dirt, and liberates no offensive gases. For the transmission of power it has the advantages over steam that it can be carried for any distance without losing any power by condensation or cooling; it is drier and cooler than steam; and in mines and tunnels, instead of rotting timbers and heating the galleries, it gives good forced ventilation by good, pure cool air. It can be applied in any direction and around corners; and, by the use of flexible pipes, may be moved as the work changes; it may be used economically and applied with little loss from friction. As an instance of this last, I may state that in the Hoosac tunnel air was carried 7150 feet in 8-in. iron pipes, its pressure falling from 67 pounds only 2 pounds. It is my intention, this evening, to narrate briefly the gradual march of air compression as a factor in the industries, and to show the principal types of modern compressors.

"Ctesibus discovered that air was compressible, and his pupil, Hero, of Alexandria, wrote a book to prove it; but from that time till Papin's date but little was heard of it. Papin proposed its use for running engines. In 1726, Rowe, in an English patent, proposed

raising water by compressed air; and, in 1753, Hall, at Schemnitz, raised water 116 feet thereby. In 1757, Wilkinson, in a British patent, suggested its use for blowing furnaces, with a series of vessels and a column of water to effect the compression. (It may be noted that the modern tendency in air compression is the use of water as the compressing medium.) In 1810, the elder Brunel, in a patent bearing his name, was vainly endeavoring to patent perpetual motion.

"In 1828, Bompas, in a provisional British patent, proposed to propel locomotives by compressed air; and, in the same year, Colladon proposed to Brunel to employ it in the Thames tunnel.

"In 1829, Mann proposed to propel fixed and locomotive machinery by air compressed in a series of pumps of successively smaller areas, calculating these areas up to the sizes needed to 64 atmospheres pressure. It must be noted that this is the origin of what is known as 'stage-pumping.'

"In 1836, the receiver, as an appendage to the compressing parts, appears; and, in 1841, Von Rathen again suggested compressed air for driving locomotives. In 1844, Coligny brings the hydraulic ram into play for air compressing; and, in the same year, Parsey patents its application for propelling carriages.

"In 1847, Von Rathen introduces the use of water for the purpose of absorbing the heat of compression. In 1852, Colladon patents its application for driving machine drills in tunnels, at present one of its most important applications.

"In 1853, Sommeiller invented hydraulic compression, or rather the use of large moving bodies of water as an adjunct to a suitable traveling piston.

"In 1853 also, Anderson patented the injection of cold water into the cylinder of the compressing pump.

"In the same year, Piatti proposed compressed air as a motive force for rock drills in the Mont Cenis tunnel; and, the year following, Parsey made the important step of putting the valves in the end of the cylinder. In 1861, rock drills, driven by compressed air, were at work in the Mont Cenis tunnel.

"In 1863, Stewart & Kershaw made a horizontal cylinder filled with water and attached to two upright air vessels, in which, alternately, the air was compressed.

"In 1864, Coughlin brings out stage-pumping, with intermediate air-vessels to give regularity of pressure; but strangely enough seems

not to have seen the still greater advantage these intermediate air-vessels would give in cooling the compressed fluid.

"In 1866, air compressors were at work in our own Hoosac tunnel, horizontal pumps being employed. In the next year, vertical pumps were substituted.

"In 1867, Doane, in this country, constructed a horizontal single-acting crank compressor, placing inlet valves in the piston-head, the discharge valves occupying the whole space across the cylinder-head, and being closed by a spring.

"In 1869, Marchant proposed to use compressed air with water for the purpose of making steam and to reheat the air, with some wild idea of increasing power.

"In 1873, Sturgeon brought out a high-speed compressor with steam and air cylinders, one each side of a receiver, and having a crank shaft with a fly-wheel; the cranks being quartered, in order that the greatest pressure of the steam may be exerted at the time of the greatest resistance of the air in the cylinder.

"In 1874, William Johnston, now of our own city, proposed a series of concentric cylinders revolving upon a fixed axis and having the lower halves filled with water, the face of which served as the bottom of the pump; and in this machine, as in the later modifications thereof, stage pumping was employed.

"In the earliest Burleigh compressor the cranks were quartered and the air cylinder vertical, in order that the valves might shut by their own weight. Cooling is effected by a water jet; the valves were circular plates; the steam piston, at first horizontal, is made vertical, and now the air cylinders are closed at the bottom and a more economical type of engine employed.

"Compressors are known as either wet or dry, according as water is or is not employed in the cylinder for lubricating and cooling. Dry compressors had at first the disadvantage that the valve could not be kept tight, and that the valve stems broke from constant slamming. These troubles are now avoided by the employment of rubber discs for valve faces and cushions.

"The well-known Dubois-François type of compressor is horizontal and employs a large body of water set in motion by a reciprocating piston, there being a large inclined inlet flap valve at each end of the cylinder and a large vertical poppet valve, for discharge, above each end.

"The Ingersoll compressor, as now made, is vertical and with cross-heads and guides and adjustable cut-off engine, this last feature increasing its capacity 20 per cent. and saving 25 per cent. of fuel. The crank is forged of iron. This compressor has one steam and one double-acting air cylinder, horizontal and side by side (each cylinder having its own bed) and the two rigidly bolted together. The inlet air valve has a slide valve closed with springs and moved by an eccentric on the main shaft and is balanced by letting the pressure from the discharge pipe into the valve box; the valves are discharge poppets arranged upon the top of the cylinder upon each end.

"In the Clayton long-stroke duplex compressor there is a water jacket completely encircling the ends first and discharging at the top, the objects being to cool the cylinders most where they are most heated and to insure the jackets always being full. The discharge valves are lifted by adjustable drop, letting the air escape as soon as it has reached the working pressure, instead of further compressing it. The cylinder is supplied with water or lubricant at each stroke, and only when working. The yoke is constructed so that the wear can be taken up in the block and in the slides at the same time. There is an air governor which keeps the pressure regular. The suction valves opening into the air cylinders have safety stems, preventing the valve from falling into the cylinder, or other accidents, if the stem breaks or the nut comes off.

"In the new Rand horizontal dry compressor the steam cylinder has poppet valves and variable automatic cut-off; the cylinders are side by side, bolted to the same bed plate; crank quartered. There is water circulation through piston and cylinder heads and around the cylinder, the cylinder having three shells, forming two annular spaces around the working cylinder. The outer space is for the air after compression, and the middle one for water circulation. The air tank receives air at one end and the discharge is through a safety valve at the other.

"In the Norwalk high speed compressor air is always taken from the outside of the engine room, to insure its being cool; increase of capacity and economy of fuel being claimed for this. The valves are drop forged; the oil cup drops oil through a wick, under pressure; combined air cylinders are employed, there being a pipe cooler between the cylinders; there is an air governor and an attachment for charging

mine locomotives, etc., by which an auxiliary compressor is thrown into action when desired. The low pressure cylinder will have say 100 square inches of area, and the high pressure $33\frac{1}{3}$, and the air crowded from the large into the small cylinder becomes compressed to 30 pounds per square inch, the resistance being $66\frac{2}{3}$ times 30, equal 1999 pounds, and ends at 100, the resistance being $33\frac{1}{3}$ times 100, equal 3333, at the same time there being a compression, as before, with 1999 pounds work done, or a total of 5333, instead of 10,000, as would be the case with the single large cylinder. By having all on one heavy cross head, the momentum stored up is given out when the the steam gets weak. The air discharge valves in a single cylinder compressor have the full receiver pressure more than half of each revolution. In the compound the heavy pressure is upon small valves only. To lessen the effect of clearance there are two ways of doing; the stroke may be made long and the piston run close, which is expensive, and requires careful watching or the space may be filled with water, which prevents high speed, as the water gets churned to a foam, besides making ice in the exhaust of the drills or other machine using the air. The water jacket has not time to fully cool the air, even if it were not rapidly covered by the advancing piston; but it is a valuable auxiliary.

"In the patent granted to Mr. W. P. Tatham, the President of this Institute, there is a steam and an air piston, each reciprocating in a cylinder, and a double armed rock shaft, connected with the steam and air piston rods, these members being combined for joint operations to compress air under a decreasing leverage of the air piston arm, and a correspondingly increasing leverage of the steam piston arm.

"In the indicator card of an air cylinder shown, the discharge valves are lifted automatically, but there is a heavy loss caused by the absence of water from the cylinder as the compressed air not discharged at the end of the stroke follows the piston back about one-fifth of the stroke. The card of the steam end of the Clayton compressor is taken at 90 revolutions, 70 pounds boiler pressure cutting off at 52 per cent., and giving an air pressure of 100 pounds.

"As far as possible, a good compressor should be simple in construction, have large valve area, be light running, compress at low temperature, have little wear and tear of working parts, be cheap in first cost, economical in use of steam or other power and of cooling water; be compact, need light foundations and be free from breakage.

It is in most cases well that the lines of motion should be the same and the clearance space small."

Mr. Henry M. Knapp made some remarks on "Engineering Errors on Rivers and Harbors," illustrated by maps and diagrams. The subject gave rise to considerable discussion, in which Messrs. Nystrom, Cartwright and Bilgram took part.

The Secretary's report included an account of Chief Engineer Abel's Water Gauge for Marine Boilers. As a vessel at sea does not always remain on an even keel, the ordinary water gauge does not show accurately the height of water in the boilers, and this has to be estimated by the "water tender." In his anxiety to keep sufficient water over the outboard heating surface he is very apt to carry the water too high, thus causing the boilers to foam and carry water over into the cylinders of the engines. To give the water tender a true guide to the height of water in the boiler when the ship has heeled over, Engineer Abel provides the boiler with the ordinary glass water gauge, and also with a reservoir of glycerine connected with a tube of much smaller diameter set near the water gauge. The reservoir is filled with glycerine or other suitable fluid to the height it is desired to keep the water over the highest heating surface. When the ship heels over the reservoir is carried up with the boilers on the weather side. The area of the cross-section of the reservoir being so much greater than that of its tube extension, the level of the fluid in the reservoir will remain practically constant at the highest heating surface, while the height of the liquid in the tube, being on a level therewith, will furnish a guide for the height of water to be maintained in the glass water-gauge. The device is simple, and can be applied to boilers furnished with the ordinary gauges.

Mr. Wm. S. Auchincloss exhibited a curious averaging machine, or book-keeper's assistant. It was designed for use in averaging dates of purchase of spools of cotton, but can be applied to many other purposes. It consists of a grooved platform, balanced by a scale beam and pan. The weights bear to each other the ratio of one to ten and one to one hundred. There are thirty-one grooves in the platform, representing days of the months, and thirty-one corresponding divisions on the scale beam. Weights representing the amount of the purchases are put in the grooves corresponding to the dates of purchase. An exactly equal load is put in the pan, and the latter is then shifted along the scale until the two parts of the apparatus are in

equilibrium. The date from which the pan is suspended gives the *average* date to which the purchases of the month should be charged. The inventor claims that a man with very little knowledge of accounts can with this apparatus average one hundred accounts per hour. The machine can be employed also to determine many other problems in proportion.

V. Quarré & Co. exhibited Hink's duplex lamps, which have a double or two entirely separate flames, fed from one reservoir of oil. They are said to give a light of twenty-six candles, burning one quart of oil in ten hours. They have a mechanical device for extinguishing the light without blowing it out.

Theodore Bergner exhibited a very conveniently arranged upright drawing board and stand, to be used in making large mechanical or architectural drawings. The board is suspended in a nearly vertical position, and is easily adjusted. It is provided with a blade or ruler, in place of a T square, guided in its absolutely parallel movement by simple mechanism, and also with facilities for taking and transferring measures and for keeping all required instruments within easy reach of the draughtsman.

Dr. Grimshaw explained a valve from a Wheelock engine, which showed very little wear, although it was said to have been in use for six years.

Mr. Washington Jones said that the valve was almost precisely like one that was put in the steamer *Adriatic* some time ago, and proved a complete failure.

Dr. Grimshaw said he knew nothing of the *Adriatic* valve. As regards the wear of the Wheelock valve, it and the worn Corliss valve accompanying it were sent to the Institute by Mr. Wheelock, with the endorsement from the Lowell Manufacturing Company.

Mr. Bilgram exhibited an ingenious little machine for making wire staples to serve as loops for hanging up pamphlets. The wire is brought together in loop form, the two ends are twisted together and bent into shape in one revolution of the driving wheel, seven operations being performed by four cams at the rate of sixty staples per minute.

The President then announced the Committee to solicit Subscriptions for the Building Fund.

There being no further business, the Institute adjourned.

ISAAC NORRIS, M.D., *Secretary*.